PROCEEDINGS

of the

SOILS AND FOUNDATION CONFERENCE

of the

U. S. ENGINEER DEPARTMENT

June 17-21, 1938

Boston, Mass.

CORPS OF ENGINEERS, U. S. ARMY

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FOREWORD

The Chief of Engineers, in a letter dated May 2, 1938, directed that a conference be held in the Boston Engineer District on June 17-21, 1938 under the direction of Colonel A. K. B. Lyman, District Engineer, for the purpose of planning an investigation and research program on Soils and Foundation problems of concern to the Department.

Captain James H. Stratton, Corps of Engineers, was designated to act as Chairman of the conference, and Mr. T. A. Middlebrooks of the office of the Chief of Engineers as Recorder. Professor Arthur Casagrande of Harvard University, Dr. Glennon Gilboy of Massachusetts Institute of Technology, and Mr. Irving Crosby, geologist, were engaged as consultants to assist the delegates in the formulation of a program. Dr. Arthur C. Ruge, assistant professor of Engineering Seismology at Massachusetts Institute of Technology, Dr. M. Juul Hvorslev, research engineer for the American Society of Civil Engineers, Mr. D. W. Taylor, assistant professor in Soil Mechanics, Massachusetts Institute of Technology, and Dr. J. P. Rutledge of Purdue University, also participated in the conference.

The first two days of the meeting were devoted to a review of the investigations and studies of cohesionless soils by the Boston District and with the presentation of the results of investigations and studies of soil problems at Harvard University and Massachusetts Institute of Technology. The last two days of the conference were utilized for discussion by the delegates of the problems pertaining to a specific research program. The papers presented to the delegates on the first two days and the record of the discussion by the delegates on the last two days are reproduced in this volume.

At the date of publishing these proceedings the following action on the recommendations of the conference has been taken by the Office, Chief of Engineers.

a. A circular letter is being prepared on the dissemination of information.

b. A meeting was held in Washington in January, 1939 to outline the research work to be done in the immediate future.

c. Investigation of soil sampling method and equipment has been started and Dr. Hvorslev has been employed as a consultant in this connection.

d. Plans are being made to install the necessary hydrostatic pressure cells or piezometer tubes in all the Department's dams.

e. Plans are being made for holding another Soils and Foundations conference next year.

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Colonel A. K. B. Lyman, C. E.
District Engineer, U. S. Engineer Office, Boston, Mass.

INTRODUCTORY REMARKS BY COLONEL A. K. B. LYMAN

I take this opportunity to welcome the representatives of the various Districts and Divisions in the Engineer Department and other guests present to hear the results of the investigations of the cohesionless soils to be used in the construction of the Franklin Falls Dam. These studies were conducted by Dr. Casagrande for the Boston District. In addition to Dr. Casagrande, you will hear from Dr. Gilboy, Dr. Hvorslev, Mr. Taylor, and Mr. Horton of the Boston District, who will discuss the subjects on the program.

The Engineer Department delegates have been advised of our program for Sunday, Monday and Tuesday. On Sunday there will be a stag excursion to the Cape Cod Canal. This excursion will, I hope, serve as a warming-up period for our conferences on Monday and Tuesday, when we will discuss an investigation and research program for the Engineer Department. This excursion was not planned solely for the purpose of providing recreation. It is not our intention, however, to deprive you of any pleasure you may derive from it. I would like to feel as a result of our trip to the Canal that we shall all become better acquainted and that some understanding of each other's problems will develop.

The problems attendant to the compaction of the cohesionless soils proposed for use in the Franklin Falls Dam were recognized by the Boston District when exploratory operations were commenced. It was natural that we should have turned to Dr. Casagrande for advice, since he had but very recently published his findings on his investigations of fine cohesionless soils. The investigations are well described in the Doctor's report and will be discussed more fully by him at this conference.

The Franklin Falls studies conducted by Dr. Casagrande produced results even of greater value in their general application than their immediate importance to this project. The development of the tri-axial compression machine gives us a new tool for the investigation of the stress-strain characteristics of sands and opens the possibility for wider knowledge of the stress-strain relationship in other soils. It appears quite probable from an examination of the laboratory results to date that the tri-axial machine may replace the direct shear machine and further our knowledge of the shearing strength of soils. It is of these matters that you will hear further from the gentlemen who are to follow me.

The word "research" connotes to many people a delving into the mysterious. Certainly we would not dispute this definition as applied to the physicist splitting the atom. As a practical people, we are prone to think of the laboratory as something apart from the job rather than an adjunct to it. Necessity and experience, however, have led the soils and foundation engineer into the laboratory. This necessity of which I speak does not have its origin in the desire of abstract knowledge but rather arises out of the real need for a better understanding of our perplexing soils problems. The field and laboratory research, which is prompted by a compelling need, more often than not pays real and immediate dividends and adds measurably to the general knowledge.

Research with a purpose, such as that conducted in the field and in the laboratory on the Franklin Falls soils, is essential if we are to refine our engineering guesses. The result of opening our minds to the importance of thorough laboratory and field investigations has been a new appreciation of the problems which confront us in our foundations and in the use of soil as a construction material. The factor of judgment has not been displaced by soils research. Judgment fortified with an understanding of the stresses and strains which act in earth structures or are created by engineering structures on soil foundations is more than ever a requirement of the soils and foundation engineer. "Experience" is merely a single word definition for an accumulation of useful knowledge. You note I say "useful." Mere exposure to engineering activity is not sufficient for the engineer specializing in soils and soil foundations. He must keep abreast of the times by current reading, and by study and observation of the technique and practices of others. This requirement or demand on the engineer has become increasingly important in engineering, and is particularly important in the flood control engineering in the Engineer Department. We find the best sites for dams have been taken for other purposes, leaving us comparatively little choice as to location. As our choice narrows, our problems in foundations and the materials for construction multiply. The complexity of these problems demands more and more from the engineering profession. A rare opportunity is afforded the engineer who is permitted to attend conferences such as this one. Today you are exposed to the influence of recognized leaders in the field of soils and foundation engineering. The advances which have been made in recent years in this field of engineering are in no small measure the results of the work of the gentlemen who will address you. They will open their minds to you, and, in fact, place themselves completely at your mercy for cross examination during the discussion periods. I hope that the results will be of great value in the future work of the Department in the field of soils and foundations.

THE SHEARING RESISTANCE OF SOILS AND ITS RELATION TO THE STABILITY OF EARTH DAMS

by

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INTRODUCTION

In a few days it will be just two years ago that the first International Conference on Soil Mechanics and Foundation Engineering gathered in this hall. Some of you attended this Conference and you know that it was very successful and that its influence was reflected in many countries by the formation of new organizations for the promotion of research in soil mechanics and for the dissemination of knowledge in this field. Let me express the hope that this Conference of the U. S. Engineer Department will be, within its more limited scope, at least equally successful.

The problems in Earth Work and Foundation Engineering can be divided roughly into two major groups which often overlap to a certain extent. In the first group we are primarily interested in the deformation of a mass of soil under the influence of moderate stresses which are well below the strength of the soil. A typical problem of this sort is the settlement analysis of a building resting on clay. While the settlements as such may be large and may even cause serious damage to the building, we have no right to speak of failure of the soil. It is simply that the building has not been designed to withstand the settlement due to volume changes and deformation in the underlying compressible soil.

The other main group of problems concerns the safety of a mass of soil against overstressing, that is, against failure of the soil either due to plastic flow or rupture. In these problems, therefore, we must study the shearing resistance of the soil. For after all it is the shearing resistance of soils which makes it possible to found a structure on, or to build an embankment of, a mass of individual mineral grains which are not cemented together. We cannot attempt to use water for this purpose because it has no shearing resistance. On the surface dry beach sand also seems to have very little shearing resistance. However, it is of fundamental importance that sand accumulates shearing resistance in proportion to the load which we apply, or which nature has applied in the form of overburden. There is no better way to illustrate this point than by a simple experiment which most of you have seen before.

We prepare a rather soft cushion by placing dry sand into a rubber bag. Then we remove most of the air with a few strokes of a hand-vacuum pump which we have connected to the bag, and we find that the bag has turned into a hard stone. When we let the air in, the bag again assumes its soft condition. The explanation is very simple. By removing the air we have applied pressure (the difference between the atmospheric pressure and the small pressure of the air inside the bag) on the sand. This pressure, with which the sand grains are pressed together, creates frictional resistance between the grains, or resistance to deformation. This resistance increases in direct proportion to the pressure between the grains, as was shown already by Coulomb in the eighteenth century. It is this simple law of friction which, in the last analysis, makes it possible for a mass of non-cemented mineral grains to carry the load of a building. The deeper the footings are carried beneath the ground surface the larger is the pressure, and consequently, the larger the frictional resistance between the grains. In addition to the weight of the overburden the weight of the building itself contributes to the pressure between the sand grains, and thereby to an increase in the "bearing capacity" of the ground.

The forces which produce pressure between sand grains may be invisible to the casual observer. For example, you have all noticed how differently the same sand will behave depending on whether it is dry, moist, or submerged. The dry sand appears loose, at least on the ground surface. We say it has no cohesion, because the grains do not adhere to each other. In the dry state, or when submerged, the sand offers little resistance on the surface and your foot will sink in an inch or two. But near the edge of the water, where the sand is moist, your feet will hardly create an impression. You cannot drive an automobile on dry or submerged sand, but the moist sand of Daytona Beach is an ideal race course. In this case it is the invisible capillary pressure which presses the grains together and mobilizes the frictional resistance necessary to carry fairly heavy loads.

Another type of force which cannot be seen, but which is of particular importance for the stability of dams and dam foundations, is the force created by percolating water within a mass of soil, the so-called seepage pressure. I can demonstrate this force by the following experiment: a tank is filled with fine sand and equipped with a filter at the bottom so that water can be made to flow through the sand in an upward direction, or drained in a downward direction.

The main purpose of such a tank is to demonstrate the nature of quicksand, and it is, therefore, called a quicksand device. For our purpose we fill this tank with water so that there are a few inches of free water standing on the surface of the sand. As long as the water in the sand does not flow in any direction we have to deal with ordinary submerged sand. If we try to excavate a pit in the sand we notice that the sides will not remain standing at an angle steeper than the angle of repose. In fact, for fine sand and rapid excavation the angle will be considerably smaller than the angle of repose because the water which is trapped in the voids reduces the friction between the grains by carrying part of the load, as will be explained later. If then water is allowed to percolate in an upward direction, we notice that the sides of the excavation begin to flatten until finally the entire excavation disappears. A quicksand condition develops if the upward percolation of water is sufficiently increased. On the other hand, if the water is allowed to drain through the sand in a downward direction, then it is possible to make an excavation with vertical sides, and even with overhanging slopes if sufficient care is exercised. However, as soon as the flow of water is stopped, the sides of the excavation will cave in and the hole may disappear entirely.

I have found experimentation with the quicksand device a most instructive aid in connection with teaching soil mechanics. The last-mentioned experiment especially provokes the curiosity of students and usually they are not satisfied until they fully understand this phenomenon; that means an understanding of the mobilization of frictional forces by the pressure between the grains, regardless of the source of these forces, and also an understanding of seepage forces and their possible effects on the soil. And I should like to add here that I have seen not only students and laymen, but experienced engineers and scientists who were startled by these simple experiments.

MOHR'S CIRCLE OF STRESS

Perhaps some of you will begin to wonder why I dwell with such length on these elementary aspects. It is because I have found that these simple facts are only too easily forgotten when dealing with more intricate problems. Since we have gathered here to discuss the shearing resistance of soils and considering how divergent the viewpoints are on this subject and how incomplete, at least in my opinion, our knowledge of this subject is, we might just as well start with the simplest terms, and also review various tools which we need in our studies, so that we may be able to talk the same "language" when it comes to the discussions. One of the tools which is indispensable in our studies of the shearing resistance of soils (be it in the evaluation of our laboratory investigations, or in the design of our structures) is Mohr's Cicle of Stress, or simply the Stress Circle as it is often called, and Mohr's Theory of Rupture.

Let us consider the stresses in a body which is exposed to arbitrary forces. If we wish to visualize the forces at a given point, we may assume a plane which goes through the point and then consider an infinitesimal square surrounding the point in this plane. The resultant force acting on this small area will, in general, be inclined, and we can resolve it into a normal and tangential component. By expressing this force per unit of area, we arrive at the normal stress σ and the tangential or shearing stress τ acting in this plane at the given point. If we let the plane rotate we shall arrive at different values of σ and τ for different positions of the plane. According to principles of mechanics there are three planes which are perpendicular to each other, for which the shearing components are zero. Therefore, the resultant stresses on these three planes are identical with the normal components. Of these three one is the largest and one is the smallest of the stresses on all possible planes. They are known as the major and the minor principal stresses, σ_I and σ_{III}.

The third stress has an intermediate value and it is called the intermediate principal stress, σ_{II}.

For our further consideration we assume a case in which the stresses vary only in two directions, a two-dimensional condition. If we know for a given point the magnitude and direction of the major and minor principal stresses, we can easily compute from the conditions of equilibrium (see any good textbook on strength of materials) the normal and shear components acting on any other plane which makes an angle α with the direction of the major principal stress:

$$\left.\begin{aligned} \sigma &= \frac{\sigma_I + \sigma_{III}}{2} - \frac{\sigma_I - \sigma_{III}}{2} \cos 2\alpha \\ \tau &= \frac{\sigma_I - \sigma_{III}}{2} \sin 2\alpha \end{aligned}\right\} \quad (1)$$

The quickest way for solving these equations, and at the same time the most useful for the visualization of stress conditions, is by means of the graphical method which was suggested by O. Mohr over fifty years ago. For this purpose we plot in Fig. A-1C on a horizontal axis σ_I and σ_{III} and draw a circle through these points A and B with its center O on this axis. Then we plot the given angle α at point A and intersect the resulting line with the circle, arriving at point M. Since the angle MOB is equal to 2α, according to the rules of geometry, it can easily be seen that the abscissae and ordinate of point M are identical with the equations (1). Hence, the coordinates of any point of the circle represent the normal and shearing stresses on a plane which intersects the direction of the major principal stress under the angle α. One can also readily see from Fig. A-1 that the intersection of this plane with the minor principal stress must be determined by the angle MBA = β. Thus, if we consider the prism shown in Fig. A-1A, with the major principal stress parallel to the σ -axis, the line AM becomes parallel to the plane for which we wish to know the stresses. However, if the minor principal stress is placed parallel to the σ -axis, as shown in Fig. A-1B, then the line BM will be parallel to the plane for which the stresses are represented by the coordinates of point M.

The circle of stress shown in Fig. A-1C represents a relationship of equilibrium of forces and is generally valid. However, it does not tell us whether for a given material the stress condition, shown by Mohr's circle in Fig. A-1C, represents a safe or even a possible condition. For this purpose we must know something about the shearing strength of the material. Let us assume that we have succeeded in determining, either by means of direct shear tests or triaxial compression tests, the strength characteristics of the material and that we have represented the shearing strengths for various normal pressures p by the s-p curve shown in Fig. A-2. Now, we superimpose Mohr's circle of stress from Fig. A-1C onto Fig. A-2 simply by using the p-axis also as the σ -axis, and the same for s and τ. Since s and p are really normal and shear components, such a procedure is entirely logical. Suppose that circle "I" in Fig. A-2 corresponds to the circle shown in Fig. A-1C. Then we could immediately conclude that the stress condition at the point under consideration is safe because not a single point of this circle comes even near the limiting s-p curve. There is, in other words, not a single plane through the given point on which the stresses are approaching the strength of the material. If we keep the minor principal stress constant and gradually increase the major principal stress, we arrive finally at a circle "II" which will be tangent to the s-p curve. For this circle there is obviously one plane on which the normal and shear stresses have just reached the condition of failure. Therefore, failure will take place along this plane which is determined by the angle α_c between the failure plane and the major principal stress. It is also obvious that it will not be possible to increase the major principal stress further, e.g. to reach the stress circle "III", because for that circle there are a number of planes in which the ratio of shearing stress over normal stress exceeds the maximum value which this material can stand.

Since every stress circle at the moment of failure must be tangent to the s-p curve, we can conclude that the s-p curve must be the envelope of all the stress circles corresponding to failure. Hence, we can determine the s-p curve, or rupture line, as it is often called, by making a series of triaxial compression tests starting the tests at different minor principal stresses, as shown in Fig. A-3. If the material has cohesion, we can use also the plain compressive and tensile strengths of the material because they are represented by circles which are tangent to the s-axis, since their minor principal stresses are zero. See Fig. A-3.

Depending on the characteristics of a material, the rupture line may have different shapes. In the most general case it is curved, with a definite value of the shearing strength at zero normal pressure, which is called cohesion. Fig. A-3 shows such a curve, which we find for example for concrete. For steel the rupture line is practically parallel to the p-axis, Fig. A-4, so that compressive strength and tensile strength are almost identical. In this case, rupture will take place under 45° planes.

A cohesionless sand has a rupture line which goes through the origin, and is practically a straight line, as shown in Fig. A-5. Such a material has no shearing strength under zero normal pressure. If the minor principal stress is known, the major principal stress is found from the following equation:

$$\sigma_{I} = \sigma_{III} \times \tan^{2}\left(45 + \frac{\phi}{2}\right) \qquad (2)$$

which is a purely geometric relationship. The slope of the rupture line is determined by the angle which is known as the angle of internal friction. The inclination of the planes of rupture with the major and minor principal stresses are $(45 - \phi/2)^{\circ}$ and $(45 + \phi/2)^{\circ}$, resp., which can be derived geometrically from Fig. A-5. It should be noted that the shearing stress on the planes of failure is $S = \sigma_{III} \cdot (1 + \sin\phi) \cdot \tan\phi$, that is less than the maximum shearing stress which always occurs on the 45° planes.

EFFECTIVE AND NEUTRAL STRESSES

So far we have tacitly assumed in our discussion that the normal pressures are entirely carried by the mineral structure of the soil, or, in other words, that they are "effective stresses." When applying stresses to a two-phase system consisting of mineral grains and water, it is by no means certain that the stresses which we apply on a mass of such material will be entirely carried by the solid phase. Quite often a substantial part of these stresses is temporarily carried by the water in the voids, thereby not mobilizing any shearing resistance. This part of the stresses is therefore known as "neutral stresses."

If we wish to analyze the shearing strength of a mass of soil, be it a small test specimen in the laboratory or the mass of a large earth dam, we must always start with an investigation of the effective stresses and of their possible change in the course of time. Only if our analysis is based on the effective stresses can we hope to arrive at an intelligent answer to our problems. If we are satisfied to use the total stresses in our computations, then we shall never be able to understand the many conflicting results obtained with shear tests on fine-grained soils, and our designs based on such assumptions may be worse than no design at all.

From these remarks it becomes clear that we must know the relation between volume of a mass of soil and applied stress, and its permeability. In other words, we must know its consolidation characteristics if we wish to determine how much of the external forces will be carried by the pore water in the form of neutral stresses, and how much will be effective stresses which produce shearing strength. It should also be evident that we cannot hope to arrive at any intelligent results if we take a correct s-p curve and apply it to the total stresses in the dam or dam foundation. If it is difficult to determine the percentage of effective stresses, as it frequently is, we must at least introduce an intelligent estimate of what this percentage might be.

TESTS ON COHESIVE SOILS WITH VERY SLOW LOAD APPLICATION.

Let us analyze what happens in a triaxial compression test in which we allow plenty of time for volume adjustment to take place. See Fig. A-6. We expose a soil sample to an all around pressure of σ_{III} and wait until all excess water is drained from the sample, Point A in Fig. A-6D. When this condition is reached all external forces are acting in the solid phase, i.e., they are effective stresses, while the neutral stress in the water would correspond to the original hydrostatic pressure in the stand pipe. Now we add a small increment of load in a vertical direction only, and wait until the volume of the sample has adjusted itself to this new stress, Fig. A-6A. We repeat this process many times, always keeping the neutral stress at a very small amount. Finally, the additional stress in a vertical direction will reach a critical value; the ultimate shearing strength of the material will be reached and the sample will fail either due to plastic flow or rupture. Plastic flow occurs when the stress-strain curve shows no drop, or only an insignificant drop, after the maximum resistance is reached, and rupture occurs when a considerable drop in resistance occurs after reaching the maximum resistance.

During this entire process of increasing the major principal stress the volume of the sample was reduced from e', Fig. A-6D, and at the moment of failure it had reached a certain e_{min} corresponding to point C.

Since the method of testing was such that the external and effective stresses were nearly identical at all times, we determine the angle of internal friction of this material simply by plotting Mohr's circle of stress, or by computing the angle from the following formula:

$$\tan^2\left(45+\frac{\phi}{2}\right)=\frac{\sigma_{III}+p_{max}}{\sigma_{III}}=\frac{\sigma_{I}}{\sigma_{III}}$$

This is the procedure which we followed in most of our triaxial compression tests described in the report of the Boston Engineer Office.

In all our considerations we have not mentioned the intermediate principal stress. It is known from recent investigations by Terzaghi that the magnitude of the intermediate principal stress influences the strength of the material. In other words, the angle of internal friction would be a maximum if the intermediate principal stress is equal to the major principal stress, and a minimum in case it is equal to the minor principal stress. However, the difference between these extremes is rather small. Furthermore, the usual type of triaxial test is made with an intermediate principal stress equal to the minor principal stress, that is for the most conservative case.

TESTS ON COHESIVE SOILS WITH FAST LOAD APPLICATION.

In nature, when we deal with a large mass of soil, the time required for volume changes to take place is so very much larger than in the laboratory that full development of effective stresses cannot always take place even for sandy soils. Perhaps the best way to understand what is happening in such a case is to analyze the mechanics of a quick triaxial compression test without drainage provision, so that the volume of the sample remains constant during the entire test.

We assume again a cylindrical sample consolidated under a hydrostatic pressure, corresponding to point A in Fig. A-6D, but without any provision to facilitate the drainage by a pervious core. Then the vertical pressure is rapidly increased until failure occurs. If the stress condition at failure is now analyzed, the effective internal stresses and the total external stresses must be rigorously separated. Since this material was not allowed time to consolidate under the additional stress p, this additional stress is entirely carried by the water in the pores of the sample, Fig. A-7C. Since pressure in the water is acting in all directions with the same intensity, the pore water will now balance a horizontal force p, so that only the difference (σ_{III} -p) of the horizontal force will act as effective horizontal stress in the solid phase of the sample. Now, the internal equilibrium is disturbed, because the horizontal effective stress corresponds to a larger void ratio. Therefore, the sample will adjust itself slightly, expanding in the horizontal direction and contracting in the vertical direction, such that the total volume of the void space will remain the same. A very slight amount of horizontal expansion or "swelling" is sufficient to result in a large drop in the horizontal effective stress, due to the typical shape of rebound curves (see e.g. Fig. A-6D, dotted rebound curve through point A). The corresponding shortening in the vertical direction will correspond only to a small increase in the vertical effective stress. On the other hand, the change in shape of the sample is making it more compressible, with the result that the effective stresses in both directions are decreased, so that the net result will usually be a slight decrease in effective vertical stress (σ_{I} - p') and a substantial decrease (σ_{III} - p") of the effective horizontal stress. The stress in the pore water, the neutral stress, will therefore be equal to Δ p" = p + p'. Thus, if the effective stresses are plotted in Mohr's diagram, the full drawn circle in Fig. A-7F, and the corresponding true angle of internal friction ϕ are obtained. Or, if the true angle of internal friction were known from previous investigations, then the quantities σ_{III} and the increase p in vertical stress at the point of failure would permit the determination of p' and p". On the other hand, if the effective stresses are not considered, but the total major and minor principal stresses at failure are merely plotted, the dotted circle and the corresponding apparent angle of internal friction ϕ' are obtained. This apparent angle of internal friction may be one-half of the true angle of internal friction, and even smaller.

COMPARISON BETWEEN QUICK AND SLOW TESTS ON COHESIVE SOILS.

While, theoretically it is, therefore, quite simple to determine the quantity p' by means of quick and slow triaxial compression tests, there are several technical difficulties which complicate the problem. If the quick test is carried out too fast, the shearing strength increases due to the addition of some viscous effect. On the other hand, if the test is carried out at a slower rate, a certain amount of consolidation takes place and the results are again too high.

In a series of tests on an undisturbed, lean clay, performed at various lateral pressures, the envelope for the slow tests was a straight line through the origin with an average angle of internal friction of 35°30'. The quick tests gave an apparent angle of internal friction of 17°50' which is larger than the theoretical value of 15°40' corresponding to the case when two circles in Fig. A-7 are tangent. Hence, instead of a positive p', we find that the two circles overlap slightly and that p' should be negative. It is apparent that consolidation during the quick test, or some other factor, is responsible for these results. Perhaps the use of the large triaxial apparatus which we have just finished and which will permit the testing of samples eight times the volume of the small samples, will lead to more satisfactory test results.

The relationships shown in Fig. A-7 hold true as long as a shear compression test is carried out at the same pressure to which a sample was consolidated. Then we find that fine-grained soils have a tendency for volume decrease during the tests, which in the case of quick tests leads to accumulation of pressure in the pore water and a corresponding decrease of the effective stresses. However, when a sample has first been consolidated to a higher pressure than the pressure at which the compression or shear test is carried out, then we observe frequently the tendency for expansion. If a quick test is carried out on such an overconsolidated material, the tendency for expansion produces tension in the pore water and additional pressure between the grains. In other words, the effective stresses are actually increased if compared with those which develop during a slow test. A quick shear or compression test on an overconsolidated material, such as is frequently placed in earth dams, yields a shearing strength which is much larger than the shearing strength which will be effective after a number of years have passed and the soil had a chance to adjust itself to the permanent pressure.

The above statements are not intended to convey the impression that overcompaction of cohesive soils, as is customarily practiced, is in itself dangerous. Deliberate overcompaction has not only the advantage that it facilitates the construction of a dam, but that the dam will always have a greater resistance against rapid changes in stresses. On the other hand, these advantages are insignificant in relation to the dangers involved when the designer assumes that the shearing strength of the compacted soil will exist permanently in the dam, regardless of the effective stress which will be active when the reservoir is full.

THE COHESION OF CLAYS.

For the quick shear or compression test on an overconsolidated soil we may develop a similar diagram as in Fig. A-7F. The effective minor principal stress will be in the extreme case equal to ($\sigma_{III} + p$). There is, however, one complication for cohesive soils. If a clay is overconsolidated, there remains a slight amount of shearing resistance in excess of the value which one obtains by direct loading. It is the so-called cohesion. As I mention cohesion of clays I hasten to add that we are entering the most controversial and least understood chapter of soil mechanics.

In the past the usual procedure for determining the shearing resistance of clay was by means of direct shear tests using a standardized technique (e.g. a test lasting about one hour). When consolidation was started at the liquid limit and the shear tests were performed under the same load to which the sample was consolidated, the s-p relation was found to be a straight line through the origin. However, when the clay was preconsolidated (e.g. undisturbed clay, or stiff remolded clay which owes its strength to capillary pressures) and is tested under normal pressures less than the preconsolidation load, then the s-p curves were usually found to be approximately straight lines intersecting the s-axis at a positive value which was termed cohesion, and inclined at an angle which was designated as the angle of internal friction. While there is no objection to the mathematical expression of such test results by means of an equation of the form $s = c + p.\tan \phi$, it is entirely wrong to assume that these quantities c and ϕ are soil constants. Neither c nor ϕ in this relationship are constants but vary widely depending on the method of testing, the consolidation characteristics of the soil, etc. Since we do not know what effective stresses were acting during the test, it is not possible to compute from such results, obtained with an arbitrary technique, the true angle of internal friction of the material. If we look at the various methods which have been used and are still used for determining the "constants" c and ϕ, and if we analyze the procedures which were followed in the use of these constants in the design of dams, etc., we must admit that there is no more truth in them than in a fairy tale.

It is only during the last few years that progress was made in our understanding of the cohesion and the angle of internal friction of clays, chiefly through the researches by Professor Terzaghi and Dr. Hvorslev, and it appears that the theoretical aspects are now fairly well understood. However, in my opinion there is still extensive experimental research needed before we will know with sufficient certainty how large the really permanent cohesion in a remolded and preconsolidated clay is, after exposure to very much smaller normal stress and considerable shearing stresses. It is my opinion that the permanent cohesion, on which we can still count after fifty or one hundred years, is only a small percentage of the shearing strength which is mobilized on the virgin compression branch. Therefore, I have in the past recommended using in the stability analysis of dams only that shearing resistance on which we can count for certainty, and allow the true cohesion as an additional safety factor. Such an approach has among others the advantage that it simplifies our analysis very much and that no serious mistakes can be made if, in addition, only the permanently effective stresses are taken into consideration.

I do not wish to recommend to any one that he should discontinue whatever method of analysis he has employed in the past. But I do believe that if he follows the above method, as an additional independent approach to his problems, he will arrive at a better understanding of the possible range within which a satisfactory solution will lie. After all, we should always keep in mind that in the practical application of soil mechanics we should never attempt to give one definite answer to a problem, as e.g. in the computation of the bending of a beam, but our solutions should consist of a range within which the true value will probably lie. And to determine this range it will usually be necessary to employ not one approach but at least two different ones.

PROGRESSIVE SHEARING FAILURE OF CLAYS.

Next to the question of cohesion of clays, it is the progressive shearing failure of clays about which our knowledge is entirely inadequate. I believe that this is a field in which model research, combined with accurate determination of the stress-strain characteristics of the materials used in the model tests, has good possibilities for the advancement of our knowledge. A study of this problem is closely linked with another aspect of dam design on which we know all too little, namely, the stress distribution within the dam and its foundation.

REMARKS ON THE FORCES ACTING IN EARTH DAMS.

The most important force acting on a dam is the actual weight of the soil of which the dam is built. Since the weight of the overlying soil in the dam changes from point to point due to the triangular cross-section, shearing stresses are created in the dam which increase with its steepness and height. If the soil in the dam had no shearing strength, or if for some reason the shearing strength were suddenly reduced to a small value, the mass would flow laterally and spread like a liquid. However, if the dam does stand up as it was built, it proves that the actual shearing resistance within the mass is equal to the shearing stress produced by its weight and other forces which are acting on the dam.

Since the shearing resistance of the soil in the dam is chiefly mobilized by the weight of the material in the dam, that is, by the force of gravity, and since this force is also trying to destroy the structure, we have the peculiar situation that the same force is trying to destroy, and at the same time mobilizing the resistance necessary to prevent disruption. This double action of the force of gravity is confusing not only to students but often misunderstood by those who design such structures.

The problem of determining the stress distribution within a dam is really a very complicated one. Depending on the type of foundation the stresses in the dam may differ widely. So far we have been satisfied to make very simple assumptions which may deviate considerably from the truth. Research on this question by means of theoretical studies, model tests, and observations in the field, is needed to throw additional light on this important problem. In the absence of reliable information on the stress distribution in the dam it is essential to make two different limiting assumptions which, to the best of our knowledge, will include the true range in which the stress distribution will fluctuate in the course of time.

STABILITY OF UPSTREAM SLOPE DURING RAPID DRAWDOWN.

Many existing earth dams with relatively impervious material on the upstream side would fail if exposed to a rapid drawdown. This, of course, is not as dangerous as a failure when the reservoir is full. Such a condition may arise if a dam is built of a cohesive soil and well compacted with modern compaction equipment. In this state the material will possess a considerable shearing strength, particularly when exposed to a quick shearing test. A stability analysis, for example, carried out by means of the Swedish method of cylindrical sliding surfaces, may give, for the proposed slope of say one on 2.5, a sufficient factor of safety. In fact, it may be felt that even a steeper slope would be safe. It would indeed be safe if this shearing strength produced by artificial compaction were a permanent quantity; however, it is anything but permanent. This shearing strength is maintained by capillary pressure, and it will be permanent only if the weight of the overlying mass will eventually produce the same shearing resistance. If this weight is much smaller, then in the course of years this material will swell and reduce its shearing strength until it eventually will reach almost that value corresponding to the virgin compression curve and the permanent stress at a given point. In other words, the initial degree of compaction creates only a temporary shearing strength which is not necessarily maintained by the dam itself. The reliance upon the original shearing strength produced by the compaction creates an entirely false sense of security. I do not mean to say that I am against thorough compaction; I merely say that we must not assume in our stability analysis the shearing strength produced by compaction, but only the shearing resistance assured in the future by the dimensions of the dam.

Otherwise a dam is obtained which is very safe when constructed; but, with time, decreases in strength until failure may occur during a drawdown. In general, the safety of dams constructed largely of clay soils is very small compared with the safety of dams whose outer sections consist of cohesionless soils. I would even go so far as to say that in most cases sand on a slope of one on 1.5 is much safer than clay on a slope of one on three, or even one on four. Yet, when the customary slopes used in earth dam design are compared, it is found that there is little difference between the slopes used for sand and gravel, even rock, as compared with the slopes for cohesive soils.

Now, when I make this statement, I should qualify it by saying "properly compacted sand," because a mass of loose sand on a slope of one on 3 or 4 may, under certain conditions, fail.

STABILITY OF COHESIONLESS SANDS.

It has been observed in a few cases that a mass of cohesionless sand placed in an embankment at an angle considerably below the angle of repose, and this sand being saturated with water, has suddenly flown out due to some disturbance, particularly vibrations. This type of <u>flow failure</u> differs from the ordinary <u>shear failure</u>. In a flow failure the shearing strength of the mass is reduced suddenly to a small value and this reduction affects not only a narrow zone but an extensive mass.

Accumulations of mountain debris, essentially a heterogeneous mixture of sand, and coarser stone fragments, are also apt to start flowing like a stream of lava during periods of saturation. This type of slide is called "Muren" in the Alps where they have been the cause of serious loss of life and property. Similar debris slides occur also in other mountains.

The mechanics of flow slides is fairly simple. We know that a mass of sand exposed to deformation will expand when in a dense state and will reduce its volume when in a very loose state. I can demonstrate this to you by putting sand into a rubber bulb and connecting it to a stand pipe. When we vibrate the sand into a dense state and then press or deform the bulb we observe that the water in the stand pipe is drawn downward. However, if the sand is placed into the bulb in a very loose state, or if worked into a loose state by rotating the bulb, we observe that a deformation of the bulb will cause the water level in the stand pipe to rise.

If a deformation is produced in loose, fine sand, it decreases in volume. When the voids are filled with water and the mass is large, it will take some time for the excess water to escape. Therefore, the weight of the mass, or at least part of it, is temporarily carried by the water in the pores, while the pressure between the grains, the effective pressure, is reduced a corresponding amount; consequently the shearing strength of the material is reduced in the same proportion. As a result, the mass will change into a semi-liquid state and start to flow out. This flow may be arrested presently by the drainage of the excess water, as occurs in model experiments in the laboratory, or on a large scale it may continue until an entire embankment has spread out so far that it has practically disappeared.

It is true that such occurrences are very infrequent, but it is equally true that many existing dams have never been exposed to a serious disturbance, and that a serious earthquake might cause the failure of such dams the upstream sections of which consist of fine or medium sands in a rather loose state. I like to compare such dams, consisting of loose sands, with a pencil standing on the table. As long as the table is not jarred, the pencil will remain standing. But once a disturbance causes it to move slightly, it will continue to fall of its own weight. On the other hand, when the sand in a dam is compacted to a dense state the dam compares with a pencil suspended at its point. When a disturbance moves the pencil from the position of equilibrium, it will return into the original position of its own accord. In the same manner, when a dam consisting of dense sand is seriously disturbed, the tendency for expansion within the sand causes tension forces in the water or an increase of the effective stresses, and therefore an increase in the shearing strength. The mass seems to be bracing itself against the movement by mobilizing a higher shearing resistance than was acting before the disturbance was created.

The boundary between the range of porosities in which expansion or volume decrease occurs, is called the critical porosity (also critical density or critical void ratio). This critical value is not a constant but decreases with increasing pressure, so that the necessary degree of compaction is greater in the heavier loaded section of the dam. The determination of the critical porosity by means of triaxial compression tests is discussed in detail in the report by the U. S. Engineer Office in Boston, entitled "Compaction Tests and Critical Density Investigation of Cohesionless Materials for Franklin Falls Dam," and dated April 1, 1938. (Note: During the Conference the results of these investigations were presented in several additional lectures, and the triaxial compression test was demonstrated.)

The results of the Franklin Falls field compaction tests showed that for the sands investigated there is no difficulty in obtaining the required degree of compaction by means of modern compaction machinery. It is entirely feasible to go far beyond the required degree of compaction. While this is not necessary from the standpoint of safety, it may be recognized as a method for cutting down the cost of such dams. But before these savings can be made use of, there are larger savings to be effected when we realize that the slopes which are generally required today for outer sections consisting of cohesionless materials are far beyond those required from the standpoint of safety, except where the strength of the foundation is inadequate. We should keep in mind that flattening slopes does not necessarily mean increased safety. When an understanding of the necessity for proper compaction of cohesionless soils is realized, we shall see very much steeper dams built than are built today. Of course, this does not apply to dams consisting chiefly of clay or dams built on soft clay deposits.

Another way in which a dam may be endangered by loose cohesionless material is in cases where the foundation consists at least in part of such materials. An earthquake or some other disturbance, as driving of piles, may cause local subsidence of the foundation materials, decreasing materially the path of percolation and in extreme cases inviting foundation failure by piping. The danger of piping from underground subsidence is particularly great for rigid dams, while for earth dams it is reasonable to assume that the dam will follow any subsidence that may be forming in the foundation.

The finer-grained the material is, the greater is the probability that it has been deposited above the critical porosity. Nature deposits such materials almost without exception above the critical porosity; and serious disturbances, particularly earthquakes, may result in their liquefaction resulting in flow slides, such as happened on a very large scale in New Zealand. The accumulation of chemical wastes, often built up in huge mountains, and fine-grained deposits from mining operations constitute extremely dangerous conditions. Such materials may appear to become quite solid after a few years and yet their water content is very high and sometimes a slight disturbance may create flow slides of disastrous consequences. Only in recent years two very serious accidents of this kind have happened, one, consisting of a large deposit of chemical wastes, in Germany, and the other one in Japan, where a large waste deposit from mining operations liquefied and flowed through a valley like a stream of lava, burying villages and killing hundreds of people. There are many deposits of this kind in the world, most of which I consider extreme hazards to surrounding life and property.

In contrast to these examples, I wish to cite one outstanding case in which very fine sands and silts (which of all types of soils have the greatest tendency to develop flow slides) are successfully used for the construction of levees with rather steep slopes. These are the levees on the large rivers in China. The secret of their success resides in the method of construction. While the levees are being built, there is a large number of workmen compacting the material with long bamboo sticks which they work up and down until no more penetration can be effected. The very slow construction due to the almost exclusive use of hand labor, combined with the patient and thorough compaction of the material, creates a mass so dense that it cannot be made to flow. Many centuries, probably thousands of years of experience, have developed this method. Perhaps the present builders of these levees could not explain the necessity of this compaction, because the disastrous experiences, which have led to the adoption of this method, are long forgotten. But we may be assured that this method of construction has not been an accidental development.

Let us compare this method of levee construction in China, using materials which would be considered unsuitable by many of our engineers, with some of the levees along the Mississippi River, built with flatter slopes, of a sand which is inherently much more stable than the Chinese river silts, and which nevertheless have failed due to typical flow slides. Modern methods of levee construction are undoubtedly very economical and very fast, but at the same time we must admit that the material is just dumped loosely. That may produce satisfactory levees from many types of soils, but certainly not from uniform fine sands.

The question will naturally arise why clays never fail by flow slides. A clay also tends to reduce its volume like a sand, in fact usually more so than the loosest sand. The difference is due in part to the fact that a clay can undergo considerable elastic deformations in which the relative position of the individual particles remains essentially unaltered. In contrast to this, even a slight disturbance of a sand produces permanent relative displacements of the sand grains. In a loose sand such displacements will cause grains to lose their equilibrium and to slip into a more stable position in the pore space between the neighboring grains. If the voids were filled with air, the result would merely consist in a very small subsidence. The stresses would still be carried entirely from grain to grain and, therefore, the ability to mobilize shearing strength in proportion to the total stress in the soil would remain unimpaired. However, if the voids are filled with water, any tendency for decrease in volume will result in a transfer of a portion of the stress from the soil grains to the pore water, thereby reducing the shearing strength of the mass. Vibrations, in which the disturbing force is applied only for a fraction of a second, may be sufficient to disturb the internal structure and equilibrium of the material to such an extent that the original shearing resistance cannot be restored until a certain amount of water has drained away. And if this drainage requires some time, the mass may begin to flow out.

Thus disturbing forces acting only for a short time may disrupt the relative position of the sand grains sufficiently to produce a serious drop in the shearing strength of the material. Similar transient forces acting on a mass of clay would have no effect, unless the forces are unusually large so that they produce shearing cracks. However, it is believed that if an embankment consisting of cohesive soils is designed with an ample factor of safety against the ordinary gravitational forces, including stress conditions during drawdown, it would also be able to withstand considerable disturbances due to earthquakes without suffering any internal disruption.

EXAMPLE OF THE REDUCTION IN SHEARING RESISTANCE DURING A FLOW SLIDE.

The question naturally arises how much one should compact those sections of an embankment which consist of cohesionless soils and which are liable, at times, to be fully saturated. Is it really necessary to compact the soil below the critical void ratio? Can we really determine the critical void ratio of a material with sufficient accuracy from laboratory tests?

In the first place we must admit that our experience with triaxial tests is still rather limited. We can be fairly certain that the stress conditions at the start of a test are, for practical purposes, homogeneous. However, it is equally certain that at 20% strain the stress distribution in the outer portion (bulge) of a loose test specimen will not be the same as in the central portion of the specimen. Since the critical void ratio is derived from the volume condition at maximum stress, which for a loose sand is at about 20% strain, we should realize that our present method for determining the critical void ratio may be seriously in error. Unfortunately this error seems to result in a higher critical void ratio, that is, the error is on the unsafe side. However, at present we know too little about this error even to venture an estimate.

Let us assume, for the purpose of the following discussion, that we have succeeded in determining the critical void ratio for various homogeneous stress conditions. Then we can be certain that if the mass is compacted below this critical value, the shearing strength of the material could not be reduced by a sudden disturbance. <u>On the contrary, there will be a marked tendency in the mass to increase its resistance while disturbing forces are acting</u>. This fact in itself increases our margin of safety to such an extent that it will protect us against many uncertainties in the design of earth structures. For this reason alone, if not for the more important reason of preventing the possibility of a flow slide, one should carry compaction below the critical void ratio whenever possible.

However, if in a portion of the dam the sand is in a state above the critical void ratio, the question arises how to determine at least approximately the maximum possible reduction in the shearing resistance which may result from disturbances. For this purpose we plot first in Fig. A-8 the results of a critical density investigation in the form of a graph showing the relationship between the void ratio at the start of the test, the volume change during the test and the minor principal stress which is a constant for each test.

Since the void ratio in Fig. A-8 refers to the start of the test, we may assume it to be the void ratio within a dam before a disturbance. And if we assume the dam large enough and the disturbance to take place in a short time, this void ratio will remain also constant throughout the disturbance and any resulting movement of the mass. If we assume the void ratio throughout the entire dam equal to 0.7 (in reality the void ratio is decreasing with depth), then it follows from Fig. A-8 that point A represents the stress condition within the mass during a flow failure. In other words, a void ratio $e = 0.7$ is the critical void ratio for a mass with a minor principal stress $\sigma_{III} = 1.0$ tons per sq. ft. Assuming an angle of internal friction $\phi = 35^o$, then the shearing resistance of this mass during the flow failure will be $s = \sigma_{III}.(1+\sin\phi).\tan\phi = 1.1$ tons per sq. ft.

Thus, if a flow condition should <u>somehow</u> develop, the effective shearing strength throughout the mass will be everywhere equal to 1.1 tons per sq. ft. For a small embankment this may be ample for safety, but for a large dam this value would be decidedly on the unsafe side. To illustrate this point, we assume in Fig. A-9 a dam consisting of fine sand, with an impervious core, and a height of 150 ft. Furthermore, we assume the water level in the reservoir at the end of a drawdown period, at 20 ft. above the base of the dam; a void ratio of $e = 0.70$, a specific gravity of the solid matter $s = 2.70$, and an average saturation of the voids above the free water surface of 50%. Then the unit weight of the soil above the free water surface will be equal to 112 lbs. per cu. ft., and the submerged weight will be 62.4 lbs. per cu. ft. Hence, the maximum pressure at the base of the dam in that condition will be about 8.0 tons per sq. ft. If this pressure is carried entirely as effective pressure, then it is able to mobilize a shearing resistance of $s = 8.\tan 35^o = 5.6$ tons per sq. ft. In Fig. A-9, below the dam, is shown by the triangle 1-2-3 the maximum shearing resistance which could be mobilized by the weight of the dam, if the disturbance would occur so slowly that at all times the weight of the dam is carried directly by the soil grains at all points. However, if a disturbance produces a flow condition, then the shearing resistance along the base at the void ratio $e = 0.7$ will drop to a value of about $s = 1.1$ tons per sq. ft., so that the resistance will be represented by the area 1-2-5-4.

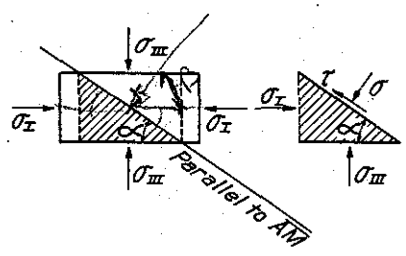
In the above example all assumptions were deliberately simplified to present more clearly the essential points. To show the effect of greater compaction on the stability of this dam, let us assume that this entire dam were compacted to a void ratio of $e = 0.6$. From Fig. A-8 it follows that the minor principal stress at the condition of flow failure for this void ratio, that is the intersection point B of the abscissae for $e = 0.6$ and the zero-volume ordinate, is $\sigma_{III} = 3.5$ tons per sq. ft. The corresponding shearing resistance in the planes of failure will be $s = 3.85$ tons per sq. ft.

By repeating the above considerations for a void ratio of $e = 0.5$ we arrive at the conclusion that the shearing resistance which could be mobilized in the base of the dam is more than 12 tons per sq. ft. For the dimensions of this dam such a shearing resistance is probably large enough to be able to resist the worst imaginable disturbing forces.

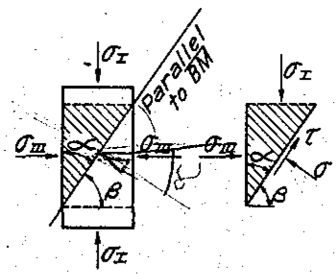
The collapse of the loose grain structure and the transfer of the pressures from the solid phase to the liquid phase may be caused by an earthquake, by a sudden subsidence in the foundation, in the case of hydraulic fill dams by excessive core pressure, etc. Our present knowledge of this subject does not permit us to say how intense such disturbances must be to produce the internal breakdown of the structure which will lead to a flow slide. This question can probably never be decided from laboratory investigations, since we cannot reproduce correctly the stress conditions in a large mass by tests. For the same reason, tests on model dams, even if of considerable size, will never be able to inform us of the conditions which will lead to a flow slide in a large dam. I have discussed this point in more detail in my paper on this subject in the January 1936 issue of the Journal of the Boston Society of Civil Engineers. In view of such difficulties we are forced to learn the factors which may produce a flow slide from careful studies of actual flow slides.

CLOSING REMARKS.

In closing I wish to impress upon you that soil mechanics is still a very young subject and still very much in the state of development. Any efforts at standardizing or simplifying the subject for the sake of teaching, or of making it easier for the practising engineer to digest, are doomed to failure for many years to come. Soil mechanics is a complex subject because the materials with which we deal are so utterly complex. In addition, to penetrate deeper and deeper into the mysterious behavior of soils, we need a much greater variety of tools than, for example, are required in structural engineering. A man who desires to work in soil mechanics needs not only a thorough knowledge of the properties of materials in general, of mathematics, and at least the principles of the theory of elasticity, but also knowledge of physical chemistry and geology, and in addition mastery of some entirely new conceptions and avenues of approach which form the fundamentals of soil mechanics.



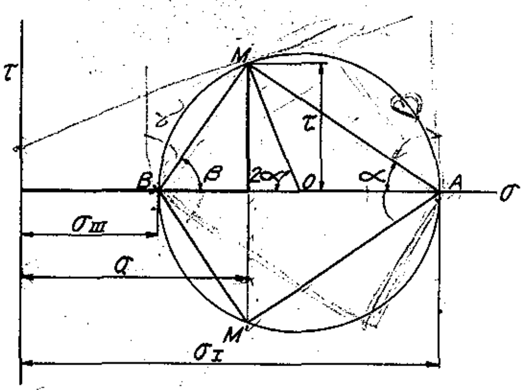
(a)



(b)

$$\sigma = \frac{\sigma_{I}+\sigma_{III}}{2} - \frac{\sigma_{I}-\sigma_{III}}{2}\cos 2\alpha$$

$$\tau = \frac{\sigma_{I}-\sigma_{III}}{2}\sin 2\alpha$$



(c)

FIGURE A-1

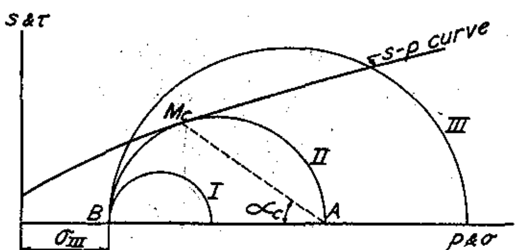
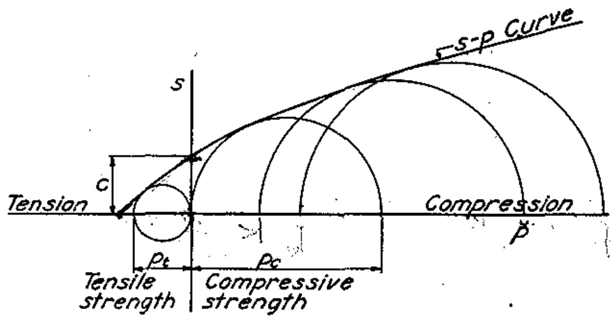
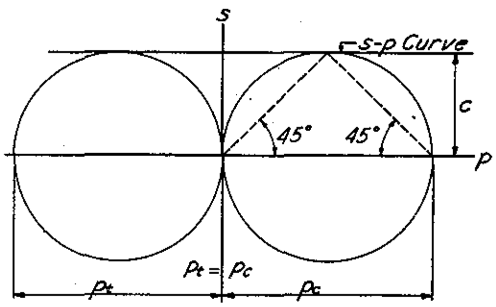


FIGURE A-2



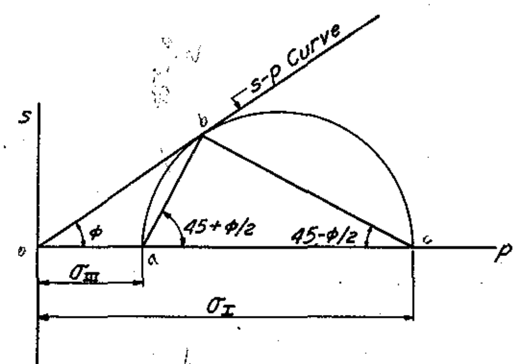
(For example: Concrete)

FIGURE A-3



(For example: Steel)

FIGURE A-4



$$\sigma_{I} = \sigma_{III} \times \tan^2(45+\phi/2)$$

$$\sigma_{III} = \sigma_{I} \times \tan^2(45-\phi/2)$$

FIGURE A-5

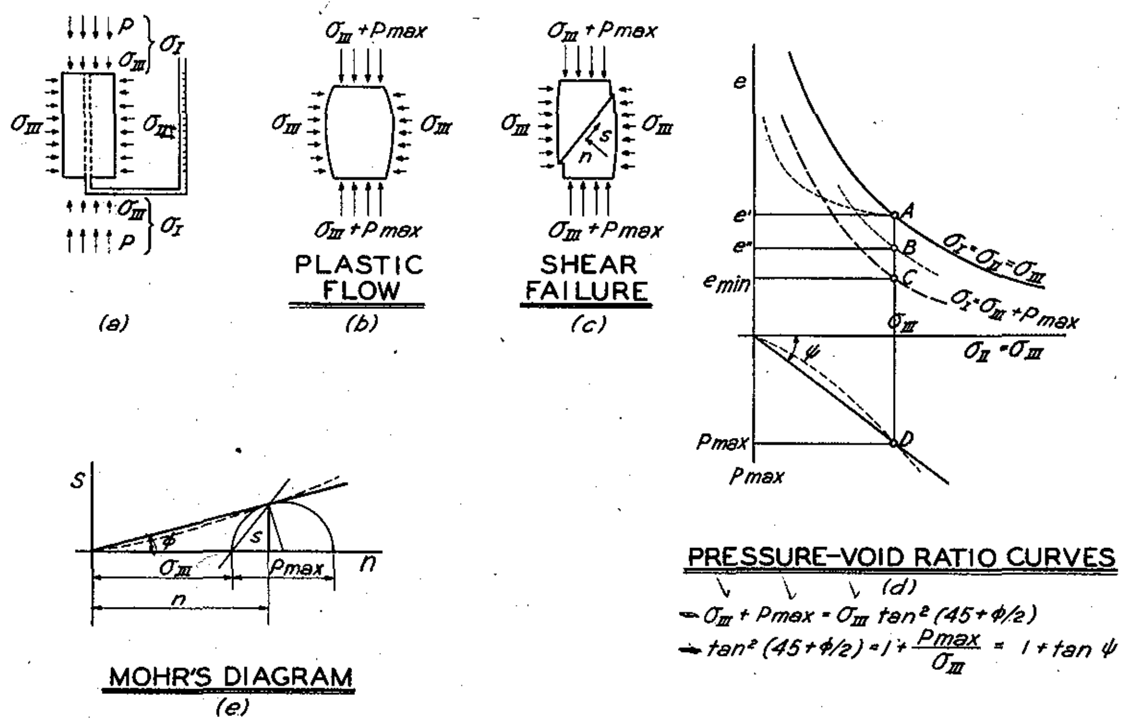


FIGURE A-6

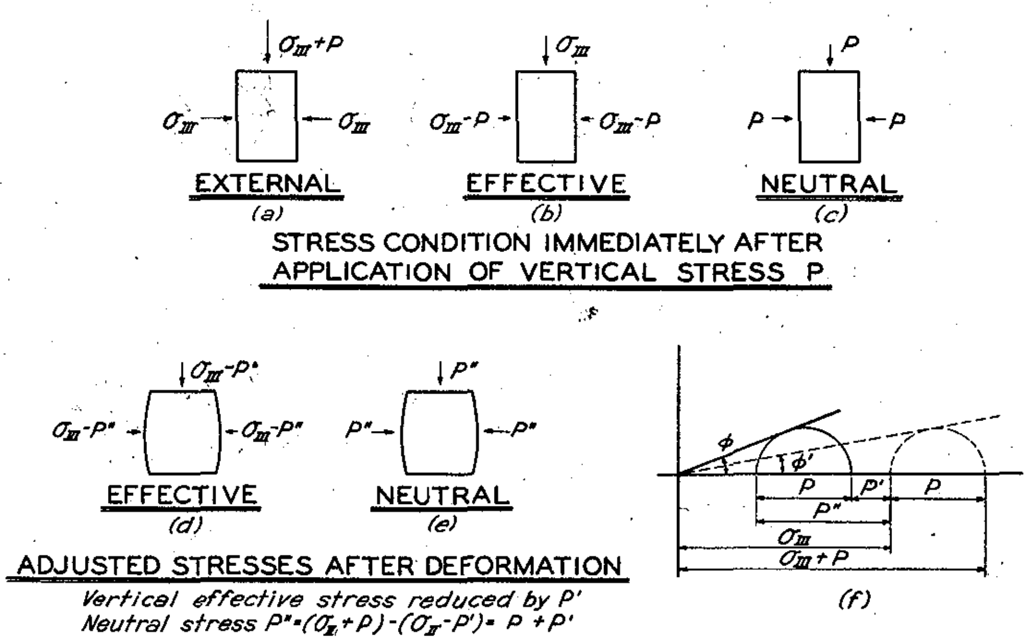


FIGURE A-7

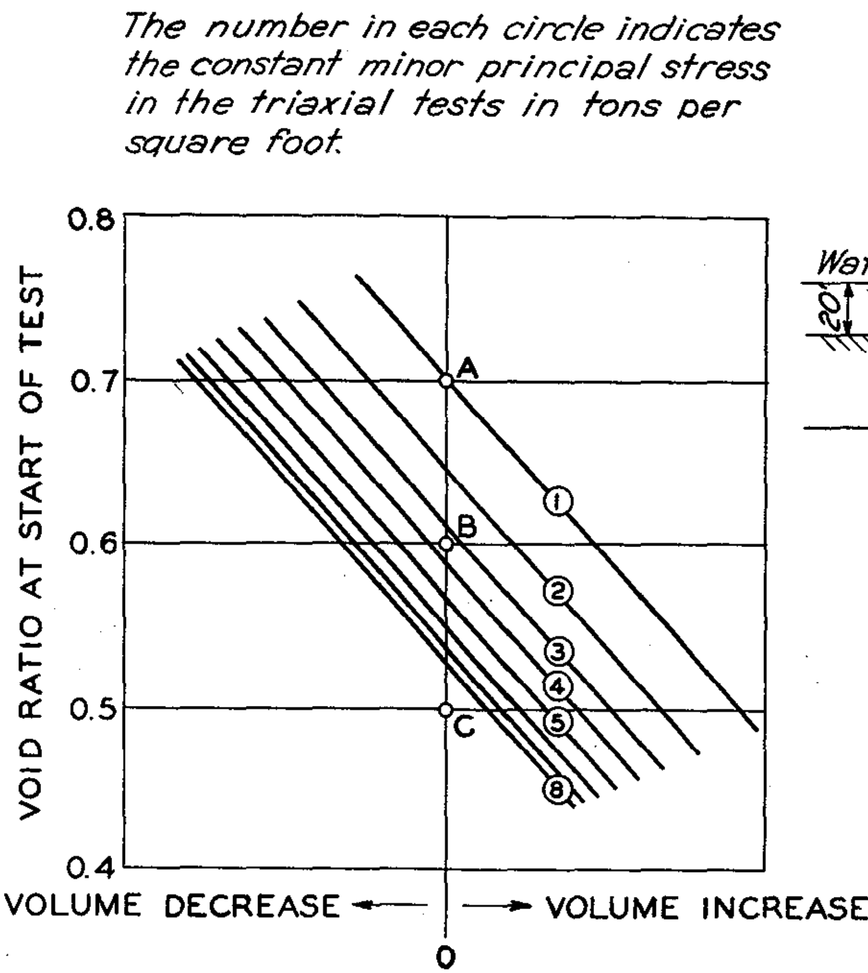

The number in each circle indicates the constant minor principal stress in the triaxial tests in tons per square foot.
VOID RATIO AT START OF TEST
0.8
0.7
0.6
0.5
0.4
A
B
C
1
2
3
4
5
8
VOLUME DECREASE
VOLUME INCREASE
0

FIG. A-8

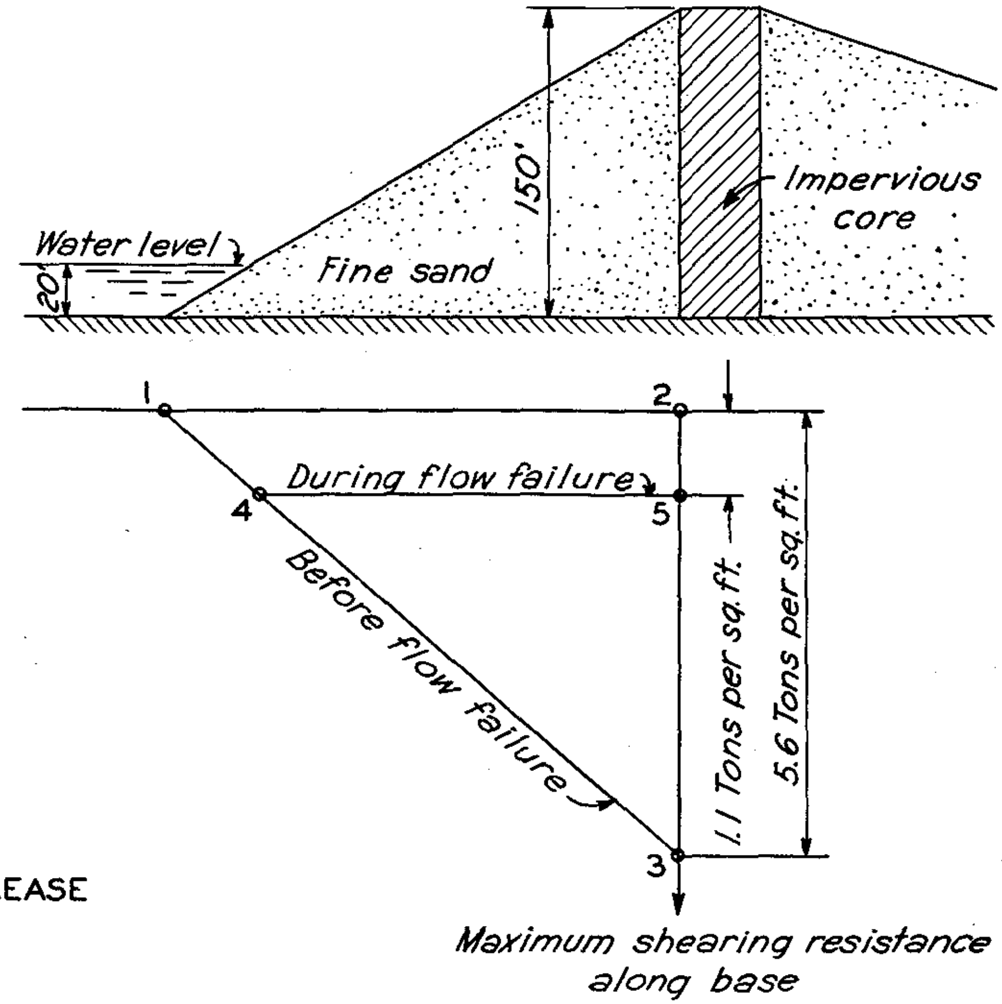

150'
Impervious core
Water level
20'
Fine sand
1
2
4
5
3
During flow failure
Before flow failure
1.1 Tons per sq. ft.
5.6 Tons per sq. ft.
Maximum shearing resistance along base

FIG. A-9

COMPACTION TESTS FOR COHESIONLESS MATERIALS FOR ROLLED-EARTH DAMS IN BOSTON DISTRICT

by

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COMPACTION TESTS ON COHESIONLESS MATERIALS FOR ROLLED-EARTH DAMS IN BOSTON DISTRICT

by

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I. INTRODUCTION

General. - Two series of field compaction tests on cohesionless materials for rolled-earth dams have been completed in the Boston District. Both series were made in connection with the design of dams for the Merrimack Valley Flood Control Project; the first for the proposed Franklin Falls Dam, the second for the proposed Blackwater Dam. I shall describe the compaction tests for the Franklin Falls Dam, illustrating certain features by slides* and a short movie, and then briefly outline the Blackwater tests in order to point out certain improvements in details of procedure followed in the latter, benefiting from experience with the former.

Franklin Falls and Blackwater Dams. - The Franklin Falls and Blackwater dams are to be similar in type. Each is designed to be constructed of a well-defined core of impervious material protected by outer sections of relatively pervious material. The pervious material will be obtained from required structure excavation for Franklin Falls and from selected borrow areas for Blackwater. The pervious earth fill in each will be further protected by filters, sand and gravel backing and rock fill. The Franklin Falls Dam will be 1,740 feet long, with a maximum height of 136 feet and will contain about 3,300,000 cubic yards of earth and rock fill. The Blackwater Dam will be 790 feet long, with a maximum height of 54 feet and will contain about 185,000 cubic yards of earth and rock fill.

II. COMPACTION TESTS ON COHESIONLESS MATERIAL FOR FRANKLIN FALLS DAM

Purpose. - The material available for the pervious shell of the Franklin Falls Dam is cohesionless with grain sizes lying in the silt and sand range. At the outset there was considerable support for the expert opinion which had been expressed that such material could only be compacted to proper density for stability by special

* Eleven slides shown, of which four have been reproduced herein (Figures B-1, B-2, Tables I, II.)

vibration equipment such as has been used in Germany. On the other hand, the difficulties inherent in requiring the use by a contractor of specially developed equipment of a type hitherto not generally used in this country are apparent. Accordingly, on the recommendation of the Board of Consultants, field compaction tests were initiated to determine the feasibility of specifying the use of standard construction machinery. Tests of tamping and vibration equipment were also made to determine whether results could be obtained with such equipment which would be sufficiently superior to those with standard equipment to warrant the use of special equipment designed to produce comparable effects.

Material. - For the tests, two types of material were selected from soil available from excavation required for the Franklin Falls Dam. One type, comprising about 60 percent fine sand and 40 percent coarse silt, was designated "Material A"; the other, a medium to fine sand, was designated "Material B". Material A was used for most of the tests because, when the tests were initiated, it was believed that, on account of its fineness and uniformity, this mixture would prove to be the most difficult to compact. It was believed that any procedure developed by the tests to give satisfactory compaction with this material would also be satisfactory for the coarser material at the site.

Critical Density. - "Critical density" was used as a criterion of satisfactory compaction. The concept of critical density and the investigation which has been made of the stress-strain characteristics of this material are described in Professor Casagrande's paper. Professor Casagrande concluded from his tests that the material in the pervious shell of the Franklin Falls Dam should be compacted to a degree of 80 percent. (The term "degree of compaction" will be defined later.)

Test Procedure. - The field compaction tests were made at the dam site. Following completion of tests on embankments constructed of one layer 4 feet in depth (designated 4-foot embankments) and of one layer 18 inches in depth (designated 18-inch embankment) further tests were made of layered embankments built up of 6, 9, 12, 15 and 18-inch layers. Samples were taken from each embankment and sent to the Boston District Soils Laboratory at Concord, N.H., for determination of water content, void ratio, and degree of compaction. The efficacy of the various methods of compaction tested was determined by comparing the corresponding void ratios and "degrees of compaction."

Degree of Compaction. - Although void ratio is an index of compaction attained, to compensate for variations in grading of the material tested, a degree of compaction scale based on "relative density" as defined by Terzaghi was also used in comparing the results of the field tests. The adopted scale is defined by the equation

$$P_v = \frac{e_o - e}{e_o - e_{100}} \quad (100)$$

in which P_v = degree (percent) of compaction; e_o = void ratio of <u>dry</u> material in loosest state; e = void ratio of material in situ in embankment or in the borrow area; e_{100} = void ratio of <u>dry</u> material in densest state.

<u>Preparation of Borrow and Test Areas</u>. - The borrow area was stripped to a depth of approximately 10 feet and care was taken throughout the tests to obtain material of the desired gradation. The work area was stripped of sod and topsoil and graded with a layer of medium to coarse sand compacted by means of the tractor used for the tests.

<u>Construction of Test Embankments</u>. - Tests with rolling equipment were made on embankments about 40 feet long, 13 feet wide on top and of varying heights. Tests with tamping and vibrating equipment were made on embankments 5 to 20 feet square on top and about 4 feet high. Side slopes were about 1 on 1-1/2, except that the end slopes of embankments for rolling equipment tests were flattened to form ramps for this equipment. A total of 43 test embankments were constructed.

<u>Control of Water Content</u>. - Tests were conducted for two general conditions of water content: (1) natural (approximating 10 percent of dry weight at the time of the tests); (2) 20 to 25 percent of dry weight. In attaining a water content of 20 to 25 percent, water was applied variously by ponding or sprinkling, suiting the method used to the requirements of the individual embankment.

<u>Compilation of Test Data</u>. - The results of the field measurements and laboratory tests were plotted to show the variation of water content, void ratio and degree of compaction with depth. After all the test results were compiled, showing variations and possible explanations thereof, general weighted curves were drawn for use in comparison of the compaction methods tested.

<u>Rolling Equipment</u>. - The rolling equipment used in the tests consisted of a heavy caterpillar tractor, smooth roller, disc roller, and three types of sheepsfoot roller, having the following weights and pressure intensities:

Item	Approximate Weight (tons)	Approximate Average Pressure Intensity (lbs. per sq.in.)
Tractor	15	9
Smooth Roller	4	26
Large-foot Sheepsfoot Roller	2	84
Small-foot Sheepsfoot Roller	2-1/2	115
*Altered Sheepsfoot Roller	2-1/2	230
Disc Roller	10	98

*Small-foot sheepsfoot roller altered by the removal of every other horizontal row of feet.

Tamping Equipment. - The tamping equipment used in the tests consisted of a drop-weight tamper, an air hammer tamper and a pneumatic hand tamper having the following characteristics:

a. Drop-Weight Tamper. Concrete block, weighing and having area similar to the Delmag-Frosch (Leaping Lena) tamper; weight about 1 ton; operated by raising 18 inches and dropping. The drop-weight tamper was designed to simulate the action of the Delmag-Frosch, but there was this important difference; the impact of the drop-weight tamper resulted from gravity alone, whereas at the moment the Delmag-Frosch strikes the ground it is driven upward by an explosive charge resulting, theoretically, in an impact twice as great as that due to gravity alone.

b. Air Hammer Tamper. Pile-driving hammer mounted on a steel sled. Assembled total weight about 5 tons; 140 blows per minute, average rate. The air hammer tamper was tractor-drawn on some embankments and placed by the crane on others. Difficulty was experienced in using this tamper when tractor-drawn.

c. Pneumatic Hand Tamper. Circular disc contact about 6 inches in diameter; frequency of tamping, approximately 4000 per minute.

Vibration Compaction Equipment. The vibration compaction equipment tested consisted of a small external and a small internal vibrator having the following characteristics:

a. External Vibrator. Electrically operated concrete surface-type vibrator; frequency of vibration, 3600 per minute. For some of the tests two 250-lb. weights were added.

b. Internal Vibrator. Pneumatically operated internal concrete vibrator; diameter, about 3 inches; frequency of vibration, 6000 per minute.

Test Results of Compaction by Tamping. - The tamping equipment tested gave results which were inferior to those obtained by either rolling or vibration equipment. The results with the pneumatic hand tamper indicate that it is suitable for compacting backfill.

Test Results of Compaction by Vibration. - The vibrators used in the tests were not themselves adapted to actual large scale operations in the compaction of earth fills, (except possibly for structure backfill compaction). The tests were intended to be indicative of the compaction which might be obtained by specially designed vibration units adapted to large scale operations. The test results indicate that vibration equipment produced degrees of compaction up to between 80 and 100 percent; saturated material compacting far better than material with a water content of 10 to 20 percent. It was found that samples of material taken from the vibrated test embankments (with one exception) had a characteristic honeycombed structure. This honeycombing was caused by the fact that, on account of the low permeability (1×10^{-4} to 5×10^{-4} cm per sec.) of Material A and the low pressure tending to cause the excess water to flow, the excess water did not escape during vibration, resulting in water voids, some of which were as large as walnuts. This is undesirable in a dam, because the seeping water might conceivably break down the honeycombed structure, possibly causing a movement of material.

Test Results of Compaction by Combined Methods. - The results of tests of combined methods of compaction (rolling followed by tamping and vibrating) indicated no definite superiority over those from rolling alone sufficient to justify the additional expense.

Test Results of Compaction by Rolling. - The data obtained from the 4-foot embankments compacted by rolling equipment were utilized in formulating the program for tests on layered embankments. These tests indicated that a water content between 20 to 25 percent was the optimum practical range for rolling equipment, the optimum water content being the greatest which will allow operation of the equipment. Attempts to use a higher water content than 20 to 25 percent were unsuccessful because the material would not support the equipment. With a water content between 20 and 25 percent the material was practically saturated after compaction.

Typical weighted compaction curves are illustrated in Figure B-1. To summarize the results for convenient comparison, use has been made of the term "estimated average attainable degree of compaction." This value was selected from consideration of the values shown by, and the shape of, the weighted compaction curves. For example, estimated average attainable degrees of compaction were taken as 106 percent for Embankment R32 and 92 percent for Embankment R33.

Estimated average attainable degrees of compaction for all the layered embankment tests with rolling equipment are shown in Table I. Note that a value of only 92 percent was obtained with 6 passes of the tractor-drawn small-foot sheepsfoot roller on 12-inch layers with water content of 15 percent, whereas 100 percent was obtained with 4 passes on 12-inch layers with water content of 20 percent. It appears logical to believe, therefore, that 6 passes of the tractor-drawn small-foot sheepsfoot roller on 12-inch layers with water content of 20 percent would have produced a degree of compaction of at least 100 percent.

TABLE I. COMPACTION TESTS ON COHESIONLESS MATERIAL FOR FRANKLIN FALLS DAM
SUMMARY OF RESULTS OF TESTS ON LAYERED EMBANKMENTS
COMPACTED WITH ROLLING EQUIPMENT (ALL TRACTOR-DRAWN)

Layer Thickness (inches)	Number of Passes	Estimated Average Attainable Degree (Percent) of Compaction					
		Disc	Large-Foot Sheepsfoot	Small-Foot Sheepsfoot	Altered Sheepsfoot	Disc	Small-Foot Sheepsfoot
		MATERIAL A				MATERIAL B	
9	2			87			
	4			93			
	6				100		
	9				105		
12	2			88			
	4			100			
	6	106*		92**	102		75##
	9				107		
15	3			93			
	6			100			
18	3			77			
	6	110#		90		70##	
	9		95#				

Notes: Average degree of compaction in natural deposit: Material A, 68 percent; Material B, 63 percent.

One pass defined as covering once with one passage of the tractor treads the area to be compacted.

Water content, 20% of dry weight except noted: *16%; **15%; #18%; ##12%.

The layered embankment test results shown in Table I indicate that estimated average attainable degrees of compaction of over 100 percent may be obtained by compacting; at optimum water content, 12-inch layers of Material A by 6 passes of either the tractor-drawn disc roller, small-foot sheepsfoot roller, or altered sheepsfoot roller. In all other results of tests on layered embankments of Material A, except 3 passes of the small-foot sheepsfoot roller on 18-inch layers, the estimated average attainable degree of compaction exceeded 80 percent.

The estimated average attainable degrees of compaction determined from the only two test embankments constructed of Material B were 70 and 75 percent, both at 12% water content. On account of the high permeability (10×10^{-4} cm per sec.) of the material, the limited supply of water available was insufficient to maintain the high water content required for best results. Although the effect of moisture upon compaction was not determined for Material B, it is logical to assume that, as was the case with Material A, optimum water content is the highest permitting operation of the compacting equipment. Consequently, it is believed that a greater degree of compaction could be attained with better control of water content than was possible in these tests. The disc roller did not operate satisfactorily on the 18-inch loose layers because it sank sufficiently to plow up the top surface, removing considerable material and disturbing the layered deposition. For these reasons it is probable that better results could be obtained in compacting Material B in 12-inch layers at optimum water content.

Conclusion and Decision. - Using Professor Casagrande's recommended degree of compaction of 80 percent as a criterion, it is evident from Table I that with Material A all methods of compaction with rolling equipment tested, except 3 passes of the tractor-drawn small-foot sheepsfoot roller on 18-inch layers, would be satisfactory. Careful study of these results and comparison with the results with Material B indicated that some of these tested methods of compaction with rolling equipment would not be satisfactory for certain possible mixtures of material available from required excavations. The selection of the recommended methods was necessarily based to a considerable extent on judgment - an important untested factor being the effect of moisture upon compaction of Material B. It was finally decided that the specifications for construction of the pervious shell of the Franklin Falls Dam should require that the available material be placed in 12-inch loose layers and be rolled at optimum water content by 6 tractor passes of rolling equipment equivalent in effect to either the tractor-drawn disc roller or the tractor-drawn small-foot sheepsfoot roller used in these tests. Additional rolling will be required wherever tests during construction indicate that the degree of compaction attained is less than 80 percent.

It is planned to confirm the conclusions based on these compaction tests by additional tests immediately prior to construction of the dam. At that time opportunity will be presented to verify certain points concerning which additional data are desired. These later tests will be made with equipment of the type which is to be used in actual construction and control will be based on the experience which has been gained from the previous tests.

III. COMPACTION TESTS ON SHELL MATERIAL FOR BLACKWATER DAM

Purpose. - The compaction tests of the shell material for the Blackwater Dam were made to determine the number of passes of the tractor-drawn sheepsfoot roller and of the tractor-drawn disc roller which would give satisfactory compaction results on selected layer thicknesses.

It will be noted that, profiting from the results of the Franklin Falls compaction tests, which were completed before the Blackwater tests were started, the scope of the program was considerably restricted. Demonstration of the practicability of securing satisfactory compaction with standard rolling equipment on the Franklin Falls fine cohesionless material made it unnecessary to test tamping and vibration equipment for Blackwater.

Test Procedure. - Field tests of compacting, in low embankments, material taken from the proposed borrow area, were made near the dam site. The efficacy of the various methods of compaction tested was determined by comparing the corresponding void ratios, unit weights and "degrees of compaction."

Equipment. - The compacting equipment used consisted of the tractor, small-foot sheepsfoot roller and disc roller used in the Franklin Falls compaction tests.

Material. - The shell material for the Blackwater Dam is a well-graded, cohesionless silty and gravelly sand, containing approximately 20 percent silt and approximately 15 to 20 percent of particles larger than 1/4 inch, up to boulders 5 feet in mean diameter.

Preparation of Borrow and Test Areas. - The borrow area and site for the test embankments were prepared in the same manner as for the Franklin Falls tests.

Construction of Test Embankments. - The embankments of shell material were constructed to the dimensions shown in Figure B-2 for III and IV (I and II were core material embankments). Each embankment section was rolled with the specified tractor-drawn roller in the direction and in conformity with the patterns shown. Three trips of the tractor and roller on each section as shown gives two passes of the roller on the strips marked for testing (i.e., where samples were taken). Hence, to make a test calling for 8 passes of the roller, 4 sets of rollings as shown are required. Two test embankments (III and IV) were constructed of seven 12" layers of the shell material. Embankment III was compacted with the tractor-drawn sheepsfoot roller, IV with the tractor-drawn disc roller. Each embankment was constructed in three sections: one section compacted by 6 roller passes; one by 12 roller passes; one by 18 roller passes.

Control of Water Content. - From the results of Proctor analyses the optimum water content for the field tests of the Blackwater shell material was taken at 10 percent. The actual water content ranged between 9.0 and 9.5 percent.

Results. - The results of the field tests are summarized in Table II. Better results were obtained with the disc roller than with the sheepsfoot roller. The compacted dry unit weights obtained with both rollers are generally greater than the maximum compacted dry unit weight obtained in the Proctor analyses (120 lbs/cu.ft.).

Conclusions. - One series of triaxial compression tests has been performed under Professor Casagrande's supervision on the Blackwater shell material. From this series the lower critical void ratio was found to correspond to about 75 percent compaction. Taking this value as a criterion of suitable compaction for stability, we conclude from Table II that the tractor-drawn sheepsfoot roller and the tractor-drawn disc roller used in the tests are both suitable for use in properly compacting the proposed shell material for the earth embankment section of the Blackwater Dam. The results indicate that 6 roller passes by an equivalent disc roller, or 18 roller passes by an equivalent sheepsfoot roller on 12" layers should be specified.

IV. CONCLUSION

In conclusion, the principal results of both series of tests may be summarized very briefly as follows:

a. Cohesionless material of the types tested can be compacted to the degree required for stability in an earth dam by standard tractor-drawn rolling equipment, the tractor playing an important part in effecting compaction.

b. In securing satisfactory compaction of these types of cohesionless material with standard rolling equipment, it is very important that the rolling be done at optimum water content.

c. Because of its importance in effecting compaction, the desired characteristics of the tractor should be as carefully specified as those of the roller and the rolling should be so controlled as to secure the required number of passes of both tractor and roller.

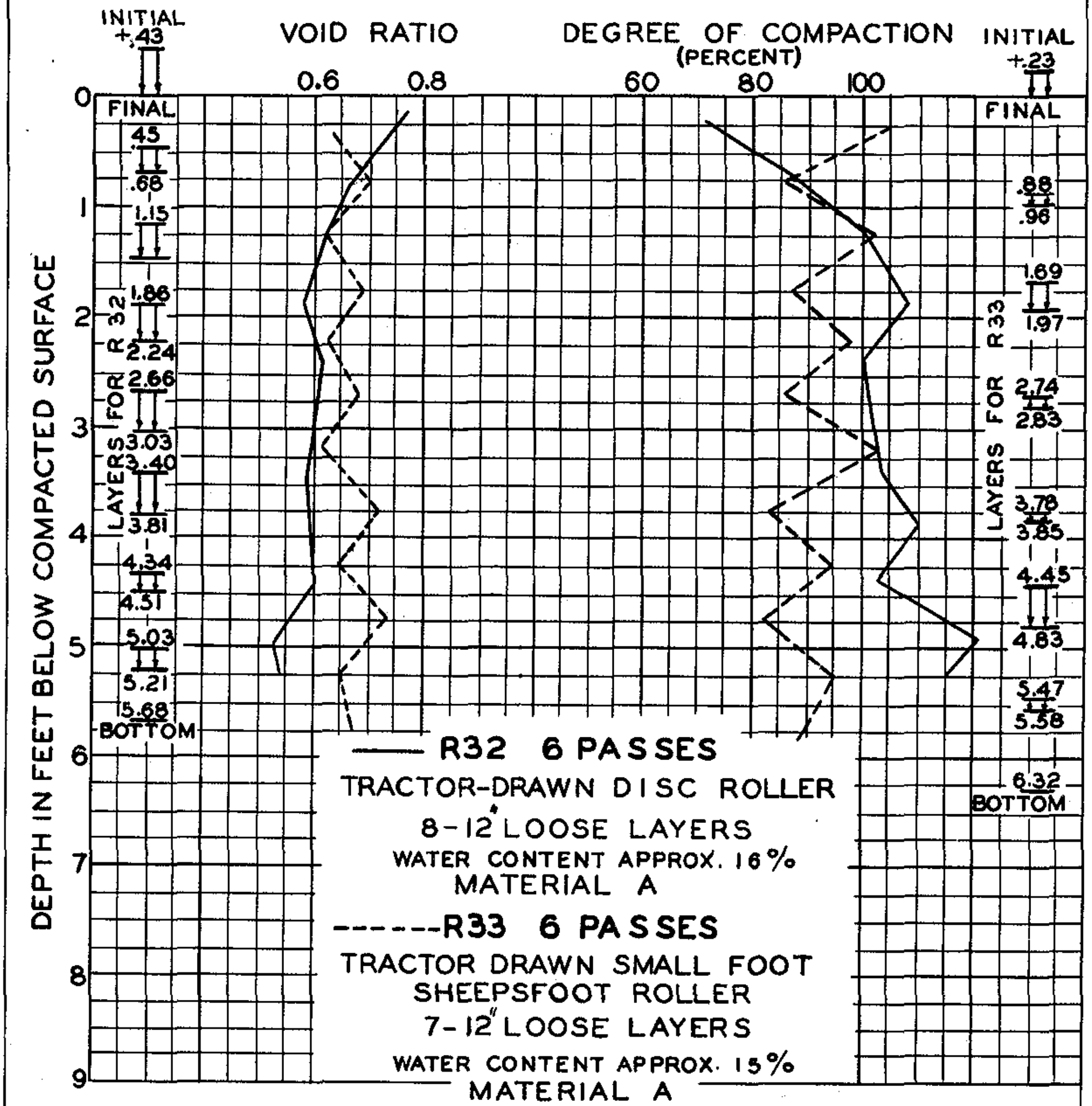
TABLE II. COMPACTION TESTS OF SHELL MATERIAL FOR BLACKWATER DAM
SUMMARY OF RESULTS OF TESTS

(FROM TABULATION OF AVERAGE RESULTS PER FOOT OF DEPTH IN TEST EMBANKMENT)

Test No.		Compaction Method		Average Dry Weight	Average Degree (%) of Compaction	Average Water Content
Embankment	Section	Roller (Tractor-Drawn)	Roller Passes			
				(lbs/cu ft)		(% of dry wgt)
III	7	Sheepsfoot	6	120	65	9.4
	8	Sheepsfoot	12	121	72	9.4
	9	Sheepsfoot	18	123	77	9.5
IV	10	Disc	6	125	90	9.1
	11	Disc	12	126	96	9.1
	12	Disc	18	128	98	9.0

Notes: All data are from tests on samples with particles larger than 1/4 inch removed.

All tests with layer thickness of 12 inches.



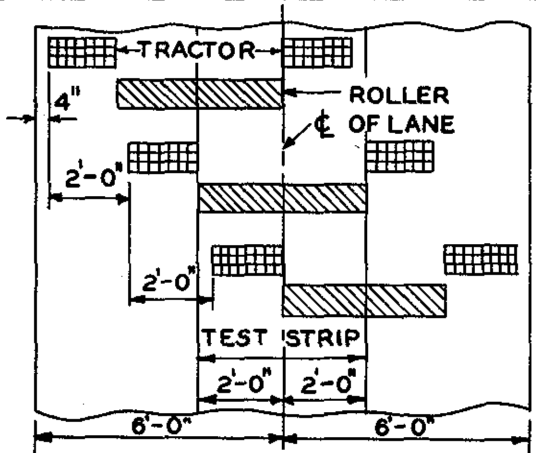
MERRIMACK VALLEY FLOOD CONTROL
FRANKLIN FALLS DAM
FIELD COMPACTION TESTS
WEIGHTED COMPACTION CURVES

U. S. ENGINEER OFFICE, BOSTON, MASS.

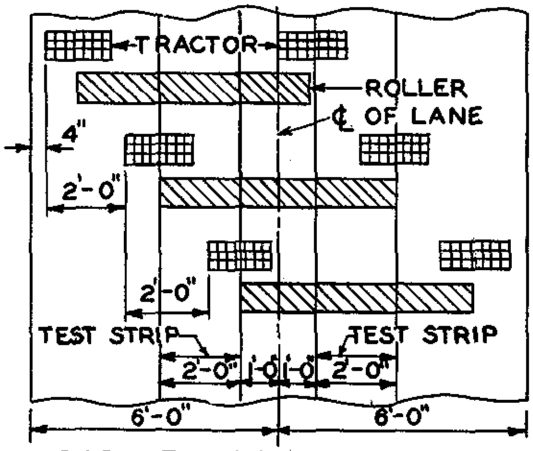
APPROVAL RECOMMENDED James W. Walton CAPT. CORPS OF ENGINEERS	APPROVED W. Shyman LT. COL. CORPS OF ENGINEERS DISTRICT ENGINEER TO ACCOMPANY REPORT DATED APRIL 1, 1938
SUBMITTED John E. Allen SENIOR ENGINEER	
PREPARED BY: Donald F. Horton ASSOCIATE ENGINEER	TR. BY P.G. CK. BY P.M. — FILE NO. M11 – 6/80

FIG. B-1

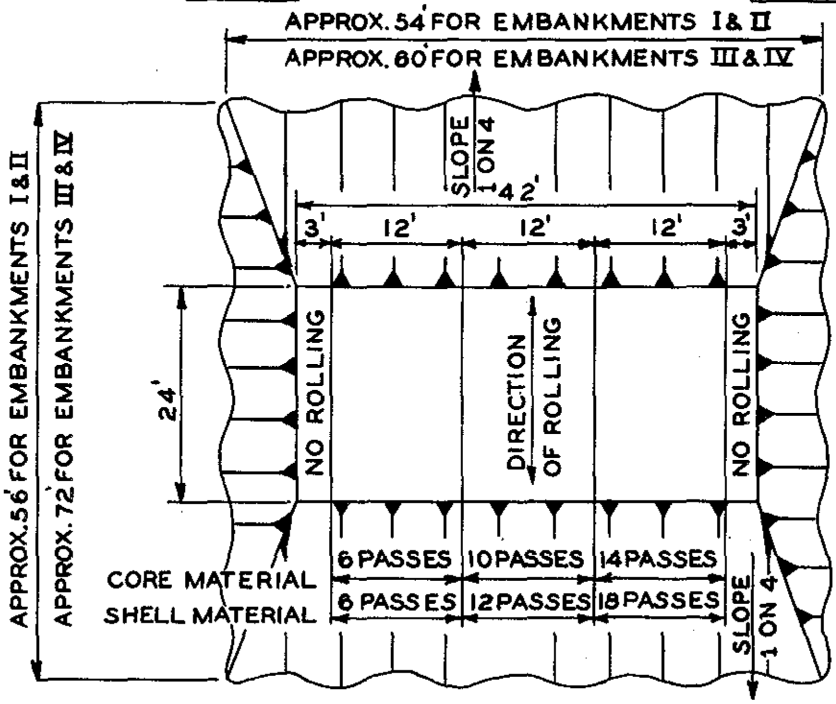
WAR DEPARTMENT CORPS OF ENGINEERS U.S. ARMY



SHEEPSFOOT ROLLER PATTERN



DISC ROLLER PATTERN



PLAN OF TYPICAL EMBANKMENT

NOTE: THREE TRIPS OF THE TRACTOR AND ROLLER ON EACH SECTION AS SHOWN, GIVES TWO PASSES OF THE ROLLER ON THE STRIPS MARKED FOR TESTING. HENCE TO MAKE A TEST CALLING FOR TEN PASSES OF THE ROLLER FIVE SETS OF ROLLINGS AS SHOWN ARE REQUIRED.

FOUR EMBANKMENTS CONSTRUCTED

- I SHEEPSFOOT ROLLER ON CORE MATERIAL
- II DISC ROLLER ON CORE MATERIAL

} 10-6" LAYERS

- III SHEEPSFOOT ROLLER ON SHELL MATERIAL
- IV DISC ROLLER ON SHELL MATERIAL

} 7-12" LAYERS

MERRIMACK VALLEY FLOOD CONTROL
BLACKWATER DAM
FIELD COMPACTION TESTS
LAYOUT OF TEST EMBANKMENTS
U. S. ENGINEER OFFICE, BOSTON, MASS.

APPROVAL RECOMMENDED CAPT. CORPS OF ENGINEERS	APPROVED LT. COL. CORPS OF ENGINEERS
SUBMITTED John E. Allen SENIOR ENGINEER	DISTRICT ENGINEER TO ACCOMPANY REPORT DATED APRIL 1, 1938
PREPARED BY: Donald F. Horton ASSOCIATE ENGINEER	TR. BY P.B. CK. BY P.M. — FILE NO. M13 - 1/2

FIG. B-2

SHEARING PROPERTIES OF OTTAWA STANDARD SAND AS DETERMINED BY THE M.I.T. STRAIN-CONTROL DIRECT SHEARING MACHINE

by

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SHEARING PROPERTIES OF OTTAWA STANDARD SAND AS DETERMINED BY THE M. I. T. STRAIN-CONTROL DIRECT SHEARING MACHINE

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Massachusetts Institute of Technology.

Present day investigations into problems of Soil Mechanics are more and more stressing the extreme importance of knowing how soils behave when subjected to shearing stresses. Thus there are at this time a number of different types of machines that are designed to test the shearing properties of soils. Still other machines are in the process of development.

Of these different types, the Direct Shear Machine in which the shearing stress is increased at an approximately constant rate has been the most common, and a wealth of data exists showing results of many years of study on this approach to shear problems. In 1934 Dr. Glennon Gilboy* of the Massachusetts Institute of Technology developed a Direct Shear Machine which had as its basic feature the ability to apply shearing displacement at a constant rate. In 1936 an improved machine of this type was designed and built at the Massachusetts Institute of Technology. In 1937 the original bellows unit for measuring the shearing force was replaced by a proving ring unit by Mr. H. A. Fidler. This machine has been described in an article by Mr. Fidler** and is the machine used in the research described in this report.

The machine of the strain-application type affords two important advantages: first, an accurate control at the important point of maximum shearing stress; second, the opportunity for study of soil behavior after this maximum point has been reached. In addition to these two there are numerous other advantages and refinements of operation.

It is generally admitted that the main disadvantage in all types of direct shear machines rests in the fact that failure is induced by progressive action, while the ideal condition would be one wherein uniform stress occurs throughout the sample. This ideal is more nearly approached in another type of machine which at M. I. T. is called the "Cylindrical Compression Apparatus", and which has also been called the "Tri-Axial Compression Apparatus". While the cylindrical compression apparatus gives promise of great value in the

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* "Improved Soil Testing Methods" by G. Gilboy, Engineering News-Record, May 21, 1936.

** See following paper entitled: "A Machine for Determining the Shearing Strength of Soils" by H. A. Fidler.

detailed study of shearing phenomena it is believed that tests by this method will require more time and technique than those by the direct shear method. Thus it is felt that because of its very practical nature, the direct shear machine is likely to continue to be the more widely used.

This report presents the results of research to determine in detail the behavior of the strain application type of direct shear machine. Its aim is to determine the shearing properties of Ottawa sand, introducing all variables that might possibly affect test results.

Of these variables, void ratio e, and normal pressure N, have long been recognized as the two which are of fundamental importance. A great many tests were run to study the variations caused by these two variables, and the results were averaged so as to assure representative values. In this group of tests, every effort was made to keep all other variables constant. The following is a list of these constant conditions.

1. The soil used was oven-dried Ottawa Standard Sand.

2. Shearing box size was 3" x 3" in cross-section.

3. The initial thickness of all samples was approximately 0.41 inches.

4. The spacing of shearing halves of the box was kept essentially constant for any given initial void-ratio, varying from 0.025" for dense state to 0.038" for the loose state.

5. The speed of the tests was held essentially constant at 0.05 inches of horizontal displacement per minute.

In a typical test, the sand was placed in the shearing box, compacted, leveled by screeding, and the normal load applied. At this point, thickness measurements for use in the determination of the initial void ratio, were made. Next the vertical and horizontal dials were placed, the top half of the box spaced with respect to the bottom half, and then the test was begun. Throughout the test the vertical load remained constant. The motor drive maintained a rate of horizontal displacement which was essentially constant. At standardized intervals, readings were taken simultaneously of the following items: total horizontal or shearing force as given by the proving ring unit, vertical extensometer dial furnishing relative thickness of sample and horizontal extensometer dial representing shearing displacement. Tests were continued until the horizontal displacement reached 0.4 inches.

An accurate determination of the initial void-ratio was a very important requirement. Since the horizontal cross-section of all samples and the specific gravity of the sand grains were constant, the void-ratio at any time could be determined from measurement of sample thickness and weight. Weights were taken to the nearest 0.05 gram. After applying the normal load the thickness was obtained by measuring the height of the loading block with respect to the base with a .001" extensometer dial, and subtracting the thickness of the loading block and toothed gratings. With this setup, relative void-ratios could be measured with a maximum error of about 0.008 for a typical value of initial void-ratio of about 0.600. The projecting teeth of the grating add a slight uncertainty to void-ratio conditions but these irregularities do not alter the basic relationships subsequently plotted, and at worst could only cause a small constant correction to all values of void-ratio.

A check on the run of the vertical dial during the test was important and was obtained by a determination of the sample thickness at the conclusion of the test for comparison with the initial thickness.

Various void-ratios were obtained by a simple method of compacting by striking the sample with a weight. Each blow was that caused by a 28 oz. weight falling $2\frac{1}{2}$ inches and striking a 3" wooden cube that rested on the 3 inch square sample.

A record was kept of blows and resultant void-ratio and Fig. C-1 shows the average curve for each normal load. No logical explanation has been found for the gap between the 3 and 4 ton per sq. ft. loadings. However, the curves do give an indication of the results of vibrating a uniform sand, starting at its loosest state. It should be stressed that these curves were not used for determination of void-ratio. Direct measurement of sample thickness and weight was always employed.

Undoubtedly Ottawa Sand could be compacted to a void-ratio somewhat lower than that obtained by 40 standard blows. However, it is difficult to conceive a method of placing the sand at a void-ratio higher than that obtained at zero blows as utmost care was taken to set up the sample as loosely as possible.

As has been mentioned, the sample was always screeded off perfectly level after the compacting operation and before placing the toothed grating. The slight disturbance caused by these operations was probably inappreciable as the experience of a few tests indicates the correct amount of sand to be used and thus excess screeding is eliminated. In all events, this disturbance could have little or no effect on the void-ratio in the important zone near the shearing plane at mid-depth.

Discussion of Results

The conventional way of representing direct shear tests is by a plot of the ratio of shear to normal stress against the horizontal displacement as shown in Fig. C-2. The maximum value of this ratio will be recognized as the coefficient of friction and as the tangent of the friction angle, ϕ. The curves show to advantage the following relationships: that the peak value of this ratio, $\frac{S}{N}$ maximum, decreases as initial void-ratio increases; that the peak is reached at horizontal displacements that increase with increase of initial void-ratio; that the curves tend to reach an ultimate $\frac{S}{N}$ at a horizontal displacement of about 0.2 inches; that the ultimate $\frac{S}{N}$ has a value which is essentially independent of void ratio or normal load.

Other typical plots are found on Figures C-3 and C-4. These graphs show the variation of sample thickness in relation to the relative value of shearing strength at which they occur and in Fig. C-4 in relation also to the horizontal displacement.

In Figure C-5, for all tests in which the normal pressure was 3 tons per sq. ft., the maximum friction angle, ϕ_m, is plotted against initial void-ratio, e_o. From the representative curve drawn in this figure it is seen that some points vary appreciably from the curve. However, the maximum variation of any point is a ϕ difference of about 0.7 degrees and it is evident that the curve is closely defined.

Similar curves were drawn for each normal pressure after which all were assembled to give Fig. C-6. A small amount of adjusting was then resorted to in order to make the curves fit together logically but in no case did this adjustment amount to more than a few tenths of a degree of friction angle. An alternate method of presenting the results is used in Fig. C-7.

The nests of curves of Figures C-6 and C-7 clearly show the familiar relationship that for any given value of normal pressure the friction angle decreases with increase of initial void-ratio. They also indicate another relationship which is at variation with that found in the past by numerous investigators; namely~~, that~~ at a given initial degree of compaction the friction angle increases with decrease of normal pressure. In research carried out on the stress-control type of direct shear machine it has generally been found that for a given degree of initial compaction the friction angle was independent of the normal load. It is shown by Fig. C-1 that the initial void-ratio depends not only on degree of compaction but also on the normal load. A typical comparison showing interrelations for various compactions and initial void-ratios is given in Table I. To obtain this table, column 3 was determined from columns 1 and 2 by the use of Fig. C-1.

TABLE I

Number of Blows	Normal pressure tons/sq.ft.	Initial void-ratio	Friction angle
		e_o	ϕ_m
5	1	.610	31.4°
5	6	.584	29.3°
25	1	.584	33.3°
25	6	.554	31.8°

after which column 4 was obtained from columns 2 and 3 by the use of Fig. C-6. The data in the table show that at a given initial void-ratio the difference in friction angles for 1 and 6 tons per sq. ft. is 4 degrees, while for a given compaction the corresponding difference is of the order of 2 degrees.

In contrast to the maximum friction angle, which varies greatly with both initial void-ratio and normal pressures, is the ultimate friction angle which has an essentially constant value. In Table II the ultimate $\frac{S}{N}$ values for all tests are summarized. It is seen that the average values for all normal pressures are within about 1% of the overall average of 0.502 which corresponds to a friction angle of 26.7°. About 40% of the individual tests vary from the overall average value by less than 1% and only about 15% vary by more than 5%.

TABLE II

Ultimate Values of Coefficient of Friction, f_u

Normal pressure tons/sq.ft.	Number of tests	Maximum	Minimum	Average
.5	5	.529	.465	.508
1	19	.521	.480	.507
2	20	.530	.472	.508
3	18	.519	.484	.502
4	23	.515	.471	.501
5	16	.520	.490	.504
6	25	.510	.486	.496
8	8	.514	.481	.498
All tests	134	.530	.465	.502

As has already been mentioned the ultimate value is reached at a shearing displacement of about 0.2 inches. For larger displacements the value of $\frac{S}{N}$ sometimes remains nearly constant but in general it decreases at a slow but constant rate. Little significance has been attached to the curve beyond this so-called ultimate point, however. The curve would be expected to remain horizontal and the small falling off may be due mainly to excessive distortions of the sample leading to poor testing conditions.

Whether the curve remains flat or falls off beyond the ultimate point, there is a quite definite point of tangency at approximately 0.2 inches displacement. At very nearly the same displacement the vertical dial and the sample thickness reach a maximum value, the sample thickness showing a similar tendency to decrease somewhat at larger strains. It would seem reasonable to assume that at this point the ultimate or critical void ratio e_c, has been reached on the failure plane of the sample. An approximate method of determining the value of e_c is discussed later in this report.

Studies of Minor Variables

Attention was given to the minor variables which might affect the values of the results. Included among these variables were the following:

1. Adjustment of apparatus.
2. Moisture and temperature conditions.
3. Size of shear box used.
4. Type of sample holders used.
5. Method of insertion and preparation of samples.
6. Thickness of sample.
7. Spacing of the halves of the shearing box.
8. Speed of the test.
9. Conditions of building vibration.

A limited number of tests were run to study the effect of each of these variables as discussed individually below. It is to be recognized that an accurate determination of some of these variables would require large numbers of tests. Thus the accuracy of the investigations of the minor variables is not nearly so good as obtained in the main series of tests.

1. For accurate results, the necessity of keeping the apparatus in good adjustment at all times is evident. Perhaps the most delicate part of the machine is the yoke which carries the shearing load to the proving ring. In the early tests some difficulty was experienced in the adjustment of this unit and it was found that at a fixed load small differences in adjustment could affect the proving ring dial readings appreciably. A small brass saddle seats against and carries the load from the yoke to the proving ring and by applying point loads at various locations along the length of this saddle differences of readings of as large as 4% could be obtained. The only change in the details of the apparatus made during this series of tests was introduced to remedy this difficulty. The saddle which was originally mounted rigidly to the yoke was redesigned to take the load from the yoke by point bearing and to be held loosely in place when not in contact with the proving ring by two guide screws.

There is one factor which if present to appreciable degree would be undesirable, which cannot be entirely eliminated. For ideal conditions the top half of the shearing box should remain fixed in position while actually it moves by the amount of compression indicated by the proving ring dial plus the amount of strain in the pins and joints of the horizontal yoke. The vertical loading yoke thus moves at its top while fixed at the bottom and it induces a slight rotation to all parts of the upper half of the shearing box. The use of the proving ring keeps this undesirable motion to a relatively small value. Actual measurements on a test under 6 tons per sq. ft. normal load show a maximum movement of 0.025 inches, about one-half of which is compression of the proving ring. As the vertical loading yoke is about 30" in height the rotation of the upper half of the shearing box is believed to be so small that it is inappreciable.

2. Precautions were taken against variations in moisture content by using oven dried sand. Past research has indicated that the friction angle for dry and saturated conditions are essentially equal, while in the intermediate range of moisture content, apparent cohesion has an effect on the results. This affect was not investigated in this series of tests.

The range of temperature experienced in the laboratory was very small and could not conceivably have any effect on the shearing strength of the sand. However, it did affect the calibration of the proving ring which measures the horizontal load. Between 17°C to 23°C, an increase in dial reading of about 0.2% per °C was found. The temperature for this series of tests was so constant that no correction was required, but temperature corrections should be considered if a series of tests were to be run during hot weather.

3. The standard size of shear box used in these tests was 3 inches square. Several tests were run, however, on a 12 inch square

box to investigate the effect of size of a sample. In general, but little difference in shearing strength was observed. In the loose state at 1/2 ton per sq. ft. values of the friction angle were about 1½% smaller for the large box and a similar variation at lower void-ratios was indicated. However, no system of void ratio determination was set up to give accuracies for the large box comparable to those obtained in the standard box.

Any load of over 3/4 tons per sq. ft. on the large box overtaxes the capacity of the present machine and thus limits the comparison. For further study of this factor shearing boxes of intermediate sizes would be required.

4. Porous stones were used as top and bottom plates in a few tests in the place of toothed gratings. The values of maximum shearing strength obtained were slightly lower than in the standard setup, being on the order of 1 to 1½% smaller. This is readily imagined since it is apparent that the toothed grating may have more ability to distribute the normal and shearing stress uniformly throughout the sample. It is known that the greater the variation from uniform distribution, the greater is the degree of progressive action and the smaller is the difference between the ultimate friction angle and the apparent maximum friction angle. On this basis the toothed gratings appear to give a slightly more uniform stress condition and therefore are to be preferred.

5. The care used in preparing the sample appeared to be at least as important a factor as any other minor variable. It was found that results were very appreciably affected where the sample was placed in the shear box without care being taken to level off the surface. If the top grating and loading block are tilted at an angle, and therefore do not seat evenly against the loading bar, an eccentric condition of loading is obtained, and while irregularities tend to disappear during the run, the peak shearing force is reached very shortly after the start of the test and any irregularities such as this definitely tend to affect its value.

With due regard to this fact, all loading parts were machined to .001 inch to obtain perfect and level seating. Furthermore, the surface of the sample was screeded off perfectly level at the start of each test by a screed that was designed to give the desired thickness of sample.

6. The thickness of sample for most of the tests of this series was approximately 0.41 inches. A limited number of tests on samples with a thickness of 0.75 inches indicated about a 3% lower value for the maximum friction angle. This greater thickness apparently causes an effect similar to that of using smoother plates at top and bottom of sample, giving a less uniform stress distribution, greater progressive effect and thus a lower value for maximum friction angle.

Further research to determine the most advantageous thickness of samples would be of value and of great interest but a very large number of tests would be required.

7. The spacing of the shearing box which gave the most satisfactory operation was found to be dependent both on the grain size of the soil and on the void ratio. It was found that the upper half of the shear box should be spaced so that, at any time during a test run, the largest sand grain is able to slip out unobstructed. Otherwise the top half tends to "ride" the grains along its edges causing considerable irregularity of movement and erratic shearing load readings. If the spacing is smaller than the maximum grain diameter, some of the sand grains are forced part way into the opening between the sections of the box and the sample is not free to compress. No difficulty was found in knowing whether or not the spacing was satisfactory as snapping and cracking of crushing sand grains as well as jerky dial readings always gave evidence of the undesirable conditions. With the spacing larger than the grain diameter some grains are sure to escape but the number was not large and the effect of the lost volume on values computed for void-ratios was inappreciable. The compression occurring in the early stages of tests on loose samples necessitated a larger spacing for this state. For this series of tests the following spacings were found to give satisfactory results: loosest samples, .038 inches; medium density samples 0.031 inches; densest samples 0.025 inches. These spacings correspond to 3/4, 5/8 and 1/2 turns on the 20 pitch spacing screws.

8. The range of rate of shearing displacements which may be obtained on the machine is from approximately 0.1 inches per minute to 0.006 inches per minute. In general the effect of variations in this rate on test results was found to be inappreciable. For tests run slower than the standard speed of .05 inches per minute no differences could be noticed, while at fastest speeds there was no appreciable difference excepting for low normal pressures and low void ratio. Here a relatively small strain causes failure and an insufficient time for structural adjustments would show to greatest effect. Even for this extreme condition the difference in friction angle did not exceed 2%.

In the results of tests run on the stress-control type of machine the conditions with respect to this variable are very different. Actually, at the peak of the stress-strain curve, if the shearing stress is being increased at a constant rate, the rate of shearing strain becomes infinite. It therefore follows that the slowest possible test on the stress-control type of apparatus is running infinitely fast at the critical point as compared to the fastest test that can be run on a strain-control type of machine. This may be pointed out as a possible reason for expecting somewhat higher friction angles by stress-control apparatus.

9. It has been noted by other investigators that vibration may have an appreciable effect on shearing tests. Thus an investigation of the effect of the vibration of the building due to the operation of heavy machinery in adjacent laboratories was made. Tests were run in the daytime when vibration was heaviest and also at night when they were at a minimum and the effects of outside traffic were negligible. No noticeable effect on the shearing strength due to vibration could be found.

Approximate Determination of Shearing Strains and Critical Void-Ratios

One disadvantage of a curve of the type of Figure C-2 is that it gives no data on the shearing strain. The following treatment is an attempt to derive an idea of the approximate magnitude of this strain and also of the critical void ratio and the effective thickness of the zone of shear. There are a number of questionable factors which enter into this approach, the answers to which can be found only by supplementary research. In this case it is hoped that in the near future results obtained by cylindrical compression tests will furnish valuable correlative data.

The plots of Figure C-4 show that theoretical dial readings approach a maximum and remain fairly steady as displacement proceeds. This steady portion of the curve covers the stage of the test where $\frac{S}{N}$ flattens off to a straight line as shown in Fig. C-2. In the following considerations it will be assumed that at the peak vertical dial reading the ultimate shearing strength of the soil has been reached at the plane of mid-depth of the sample. Since the critical void-ratio may be described as the value corresponding to the ultimate shearing strength, it follows that for some undetermined height at the center of the sample the critical void-ratio has been reached.

At this ultimate point a line, which would have passed vertically through the center of the sample at the start of the test, has distorted to the shape indicated diagramatically by the full line ab in Fig. C-8, the entire sketch representing the cross-section of the sample. The angle between the vertical and any point on the line ab represents the angular strain at the point. The tangent of this angle is the shearing strain.

The effective height of the zone of shear, designated by H, is described as the height of sample which, if subjected throughout to a shearing strain equal to the ultimate average value at the mid-depth of the actual sample, would show the same shearing displacement. If the curved line ab is replaced by a broken line acdb and other similar lines such as a_2b_2 are treated in the same way the shaded zone of Fig. C-8 is obtained. It is suggested that the average height of this shaded zone may be taken as a rough but sufficient accurate conception of the magnitude of H.

The vertical dial run which is equal to the change in thickness of the sample, represents a vertical direct strain, the magnitude of which is at any point dependent on the shearing strain at the point. There is a small amount of shearing strain outside the shaded area of Fig. C-8 and at top and bottom of the shaded area the shearing strain is less than the maximum value at the center, while the distribution of vertical strain is similar. This leads to the basic assumption which is made relative to the quantity H, the description of which should be recalled to mind at this point. It is assumed that the ultimate change in thickness of the actual sample is equal to the change of thickness in the effective height which is equal to H at the ultimate point, the vertical strain throughout being that which corresponds to the uniform shearing stress in height H.

It is worth while to note in passing that a valuable visual picture of the progressive effect which has been mentioned is pointed out by the difference in the slopes of the lines cd and c_2d_2.

The basic formula for change of thickness of a sample in terms of change of void ratio gives the following:

$$\Delta H = \frac{H}{1+e_c} (e_c - e_o)$$

wherein H is as described above, ΔH is the change in this height or the change of thickness of sample during the test, during which time the void ratio passes from its initial value e_o to the critical value, e_c. For any given normal pressure the value of e_c is constant. The value of H may depend somewhat on e_o but it will be assumed constant at this point and the effect of any variation will be considered later. The above is therefore a linear equation in the two variables ΔH and e_o. A plot may be drawn and since ΔH and e_o are known for each test there are as many points available for the plot as the number of tests run for the given normal load. A typical plot of this type is given in Fig. C-9 for 6 tons per sq. ft. normal load while similar plots for all loads used appear in Fig. C-10. It is seen that there is some scattering of points but that the average straight line may be found with fair accuracy. The intercepts of these lines furnish the desired constants e_c and H, since when $\Delta H = 0$, $e_o = e_c$ and when $e_o = o$, $\Delta H\ e_c/(1+e_c) = H$. However, instead of determining the intercept at $e_o = 0$ it is more convenient to determine the one at $\Delta H = 0.02$ inches. The intercepts at $\Delta H = 0$ and $\Delta H = 0.02$ have been picked from the curves of Fig. C-10 and are plotted against pressure in Fig. C-11A and C-11B. The smooth average curves furnish values which have been back-plotted to give the light dashed lines of Fig. C-10. If e_1 represent the value of e_o when $\Delta H = 0.02$ then $H = 0.02\ (1+e_c)/(e_c - e_1)$. Using values from the smooth curves of Fig. C-11A and C-11B in this equation furnishes values of H which may be plotted against normal pressure as in Fig. C-11C.

It has already been mentioned that some question may arise as to whether or not it was correct to assume H a constant and independent of e_o. It should be noted that the critical void ratio determination as obtained by intercept in Fig. C-9 does not require that H be constant. If H depends on e_o the plot probably would not be linear but the intercept at $\Delta H = 0$ would be available with practically the same ease and accuracy.

Once the values of the critical void-ratio and the effective height are available the curves of the types shown in Figures C-2 and C-4 may be replotted to give the curve of $\frac{S}{N}$ vs. shearing strain and also the curve of void-ratio vs. either $\frac{S}{N}$ or shearing strain. Using shearing strain for abscissae, curves of the above type for two typical tests, one dense and one loose, are shown in Fig. C-12. In accordance with the description of H, the computations are made on the basis that there is a change of thickness in the effective zone of shear which is equal to the change of thickness of the actual sample.

The curves of Fig. C-12 point out in a very instructive way the reason why the friction angle is higher for dense state than for loose state. The void ratio always is increasing toward the critical value when the peak value of $\frac{S}{N}$, designated by f_p, is being approached. The amount by which this peak value exceeds the ultimate $\frac{S}{N}$, designated by f_u, is furnished by interlocking of soil grains. The amount of interlocking is dependent on the void-ratio which exists at the peak point, or it may be said that more directly it depends on the difference between the critical void-ratio e_c and the void-ratio at the peak, e_p. A good visualization of these quantities is given by Fig. C-12 and it should be instructive to obtain a comparison for all tests of the values $(f_p - f_u)$ and $(e_c - e_p)$. An attempt has been made to show the relationship by a plot. However, it is very evident that both quantities are small differences based on relatively large values which do not have very high probable accuracies. Thus in the plot of $(f_p - f_u)$ vs. $(e_c - e_p)$ shown in Fig. C-13 the probable accuracy of any given point is undoubtedly very low. However, all points fall between the two heavy dashed lines and noting from Table II that f_u has an average value of 0.50 the following rough empirical equation may be obtained.

$$f_p = 0.50 + 2\,(e_c - e_p) \pm 0.04$$

A dense sample has a large void-ratio change to undergo before reaching the critical value, the course of this change being shown in Fig. C-12. Regardless of what the shearing strain may happen to be when the peak value f_p is reached, it is evident that for a dense sample there must be a large value of $(e_e - e_p)$, greater interlocking and higher interlocking strength, $(f_p - f_u)$, than there would be for a loose sample.

The notation used in the above discussions and in the figures are given in Table III. A tabulation of the most important of the test results is given in Table IV, which is attached at the end of the report.

TABLE III

ϕ_m	Angle of internal friction, $\tan^{-1}$ (max. S/N)
ϕ_u	Ultimate angle of internal friction.
f_p	Coefficient of friction, equal to peak value of S/N, corresponding to ϕ_m
f_u	Ultimate coefficient of friction
e	Void-ratio
e_o	Initial void-ratio
e_c	Critical void-ratio
e_p	Void-ratio at peak value of S/N
e_1	Intercept of curve of e_o vs. ΔH at ΔH + .02"
S	Shearing stress
N	Normal stress
ΔH	Change of thickness of sample
H	Effective thickness of sheared zone

Summary and Conclusions

Using the new M. I. T. direct shear machine in which the shearing strain is applied at a constant rate and maintaining all minor variables as nearly constant as possible, an extensive series of tests were run on Ottawa standard sand at various initial void-ratios and normal pressures. They furnish the shearing strength data which is given in Figures C-6 and C-7 and Table II. A limited number of supplementary tests to study the effect of minor variables were run and in no case was it found that a minor variable affected the results by more than 3%. Initial void-ratios were measured carefully. By considerations of initial void-ratios, of changes of thickness of samples and of horizontal displacement, an attempt was made to obtain information on the critical void-ratio and the shearing strains. The critical void-ratio results are shown in Fig. C-11A and typical stress-strain curves in Fig. C-12.

The shearing strength results are somewhat lower than the generally accepted values which have been obtained by other types of direct shear machines. It was found that for a given initial density the friction angle is considerably larger at small than at large normal loads. Except for these two statements no further comparisons are made here as it is expected that results by cylindrical compression tests will be available in the near future and comparisons with such data should be of much more value than afforded by any data now available. A still more important comparison which cylindrical compression tests should furnish is in connection with the critical density. The values in Fig. C-11A are derived after introducing assumptions which may be questionable and these values should not be used for even the roughest of indications until checked by some other method.

All testing work in connection with this research was performed by Mr. T. M. Lops. The work was carried out as a staff research project in the Massachusetts Institute of Technology Soil Mechanics Laboratory. It is here presented as a progress report. More conclusive results on portions of the work such as the effect of minor variables and the material relative to critical void-ratio will be obtained through further investigation.

TABLE IV

Test #	e_o	Peak Readings		Ultimate Readings		
		f_p	Vert. dial 10^{-4} in.	Max. vert. dial, Δ H	f_u	Hor. dial
0.5-1	0.672	0.582	2.			
0.5-2	.661	.559	25.			
0.5-3	.671	.582	-10.			
0.5-4	.675	.582	14.	36.	0.497	0.200
0.5-5	.685	.565	1.	18.	.505	.150
0.5-6	.673	.575	6.	25.	.544	.170
0.5-7	.586	.670	49.	146.	.465	.340
0.5-8	.571	.700	54.	208.	.529	.190
1-1	.659	.563	7.			
1-2	.660	.566	29.	56.	0.521	.180
1-3	.675	.559	10.	25.	.486	.200
1-4	.670	.563	6.	26.	.480	.200
1-5	.671	.559	-6.	24.	.490	.200
1-6	.680	.547	-7.	22.	.516	.183
1-7	.598	.641	55.	146.	.515	.170
1-8	.601	.629	44.	140.	.515	.190
1-9	.595	.629	51.	146.	.518	.220
1-10	.601	.610	43.	136.	.503	.190
1-11	.600	.625	52.	143.	.510	.190
1-12	.571	.677	91.	197.	.497	.190
1-13	.574	.677	49.	167.	.501	.190
1-14	.584	.669	58.	169.	.500	.175
1-15	.576	.684	54.	177.	.501	.220

Table IV - (Continued)

Test #	e_o	Peak Readings f_p	Vert. dial 10^{-4} in.	Ultimate Readings Max. vert. dial, ΔH	f_u	Hor. dial
1-16	.576	.680	55.	173.	.512	.174
1-17	.608	.599	60.	138.	.521	.190
1-18	.628	.599	42.	97.	.520	.180
1-19	.625	.586	33.	101.	.503	.220
1-20	.615	.583	47.	124.	.519	.220
2-1	0.648	.563	0.	32.	.518	.176
2-2	.670	.554	-17.	09.	.519	.177
2-3	.666	.539	2.	10	.504	.190
2-4	.659	.525	7.	25.	.472	.250
2-5	.579	.644	53.	154.	.512	.180
2-6	.595	.609	57.	125.	.512	.190
2-7	.595	.609	43.	120.	.530	.153
2-8	.608	.606	30.	116.	.511	.190
2-9	.595	.609	40.	124.	.520	.190
2-10	.559	.673	59.	150.	.504	.186
2-11	.559	.689	72.	184.	.515	.180
2-12	.566	.664	59.	157.	.509	.200
2-13	.563	.669	57.	164.	.511	.200
2-14	.565	.669	64.	182.	.495	.200
2-15	.669	.535	-15.	3.	.479	.190
2-16	.671	.535	-31.	-29.	.525	.151
2-17	.610	.595	41.	102.	.516	.190
2-18	.607	.583	56.	96.	.516	.190
2-19	.570	.669	68.	158.	.499	.250
2-20	.562	.665	98.	161.	.498	.250
3-1	0.650	0.535	8.	25.	.509	0.180
3-2	.652	.524	7.	23.	.516	.178
3-3	.659	.517	3.	17.	.504	.200
3-4	.645	.542	-2.	32.	.519	.170
3-5	.669	.523	-7.	-1.	.508	.190
3-6	.669	.524	-11.	7.	.505	.190
3-7	.602	.588	32.	97.	.501	.190
3-8	.588	.588	37.	109.	.496	.190
3-9	.592	.611	57.	134.	.510	.180
3-10	.578	.619	71.	139.	.501	.190
3-11	.595	.590	35.	126.	.484	.210
3-12	.566	.626	54.	171.	.496	.220
3-13	.556	.643	82.	159.	.496	.180
3-14	.562	.654	79.	169.	.496	.220
3-15	.554	.660	64.	161.	.500	.220
3-16	.565	.629	61.	164.	.496	.190
3-17	.655	.522	-5.			
3-18	.654	.533	4.	30.	.496	.220
3-19	.664	.530	-3.	12.	.496	.220

Table IV - (Continued)

Test #	e_o	Peak Readings f_p	Vert.dial 10^{-4} in.	Ultimate Readings Max.vert. dial, ΔH	f_u	Hor. dial
4-1	.647	.541	20.	39.	.511	.220
4-2	.650	.549	8.	26.	.508	.200
4-3	.651	.535	-2.	12.	.511	.210
4-4	.645	.545	20.	47.	.512	.210
4-5	.646	.544	18.	35.	.505	.200
4-6	.580	.599	56.	149.	.514	.200
4-7	.575	.606	63.	148.	.502	.210
4-8	.576	.600	70.	149.	.494	.160
4-9	.575	.614	42.	134.	.496	.160
4-10	.582	.579	49.	120.	.486	.190
4-11	.551	.624	51.	160.	.471	.350
4-12	.551	.649	61.	147.	.491	.220
4-13	.555	.642	74.	157.	.495	.220
4-14	.555	.639	54.	144.	.491	.190
4-15	.555	.628	61.	144.	.491	.230
4-16	.656	.535	-7.	18.	.506	.190
4-17	.651	.535	12.	27.	.499	.190
4-18	.654	.538	-1.	18.	.515	.160
4-19	.658	.524	-15.	-11.	.506	.160
4-20	.589	.577	38.	102.	.503	.160
4-21	.584	.585	55.	113.	.511	.160
4-22	.547	.654	70.	164.	.492	.190
4-23	.551	.649	65.	151.	.505	.190
5-1	0.641	.532	13.	34.	.513	.190
5-2	.645	.539	0.	32.	.513	.210
5-3	.646	.529	2.	28.	.508	.240
5-4	.642	.527	17.	39.	.516	.180
5-5	.636	.550	26.	37.	.520	.180
5-6	.548	.647	83.	172.	.495	.210
5-7	.554	.639	80.	177.	.490	.220
5-8	.540	.665	76.	191.	.496	.250
5-9	.549	.636	77.	179.	.490	.250
5-10	.549	.654	52.	170.	.500	.220
5-11	.575	.598	61.	140.	.500	.190
5-12	.575	.610	57.	146.	.498	.200
5-13	.579	.598	56.	131.	.500	.200
5-14	.579	.593	48.	128.	.505	.175
5-15	.581	.584	64.	140.	.498	.190
5-16	.660	.525	-10.	10.	.515	.180
6-1	.649	.524	2.	26.	.508	.210
6-2	.649	.525	5.	25.	.491	.200
6-3	.631	.524	12.	28.	.500	.200
6-4	.642	.519	10.	38.	.493	.250
6-5	.631	.515	19.	28.	.497	.200

Test #	c_o	Peak Readings		Ultimate Readings		
		f_p	Vert.dial 10^{-4} in.	Max.vert. dial; ΔH	f_u	Hor. dial
6-6	.566	.596	49.	138.	.490	.220
6-7	.560	.615	57.	148.	.504	.160
6-8	.565	.586	64.	133.	.504	.190
6-9	.575	.586	65.	127.	.494	.190
6-10	.580	.574	60.	113.	.498	.170
6-11	.541	.647	68.	165.	.487	.240
6-12	.540	.633	73.	172.	.487	.240
6-13	.545	.629	53.	157.	.492	.220
6-14	.548	.630	50.	161.	.487	.250
6-15	.553	.630	64.	183.	.492	.250
6-16	.606	.549	20.	72.	.497	.180
6-17	.610	.539	15.	61.	.500	.180
6-18	.610	.544	28.	77.	.505	.210
6-19	.610	.530	39.	77.	.500	.200
6-20	.651	.515	-23.	-16.	.487	.190
6-21	.645	.531	2.	7.	.501	.160
6-22	.584	.568	48.	93.	.486	.180
6-23	.590	.548	27.	75.	.500	.160
6-24	.549	.626	56.	138.	.495	.190
6-25	.548	.630	58.	129.	.510	.190
8-1	.644	.510	-12.			
8-2	.545	.620	74.	153.	.481	.166
8-3	.636	.502	-15.	-8.	.487	.160
8-4	.645	.515	4.	9.	.502	.150
8-5	.639	.527	-3.	7	.512	.190
8-6	.647	.524	-6.	5.	.514	.160
8-7	.550	.631	68.	155.	.499	.180
8-8	.540	.639	48.	160.	.488	.220
8-9	.542	.636	57.	154.	.504	.220

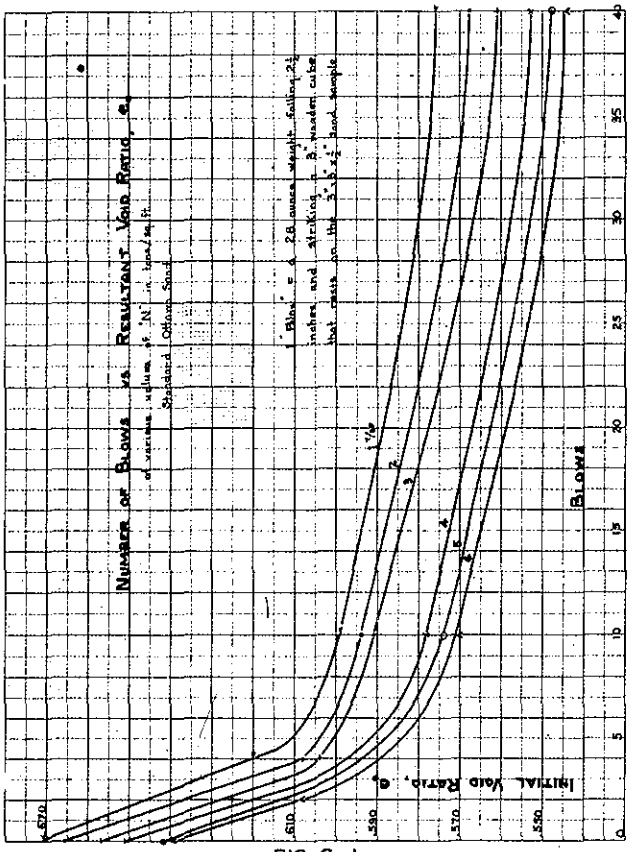

FIG. C-1

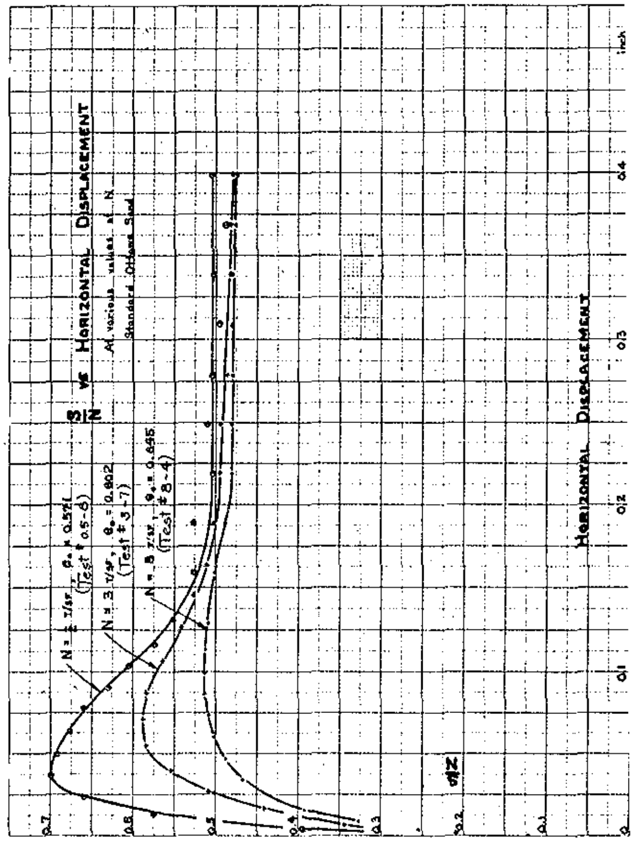

FIG. C-2

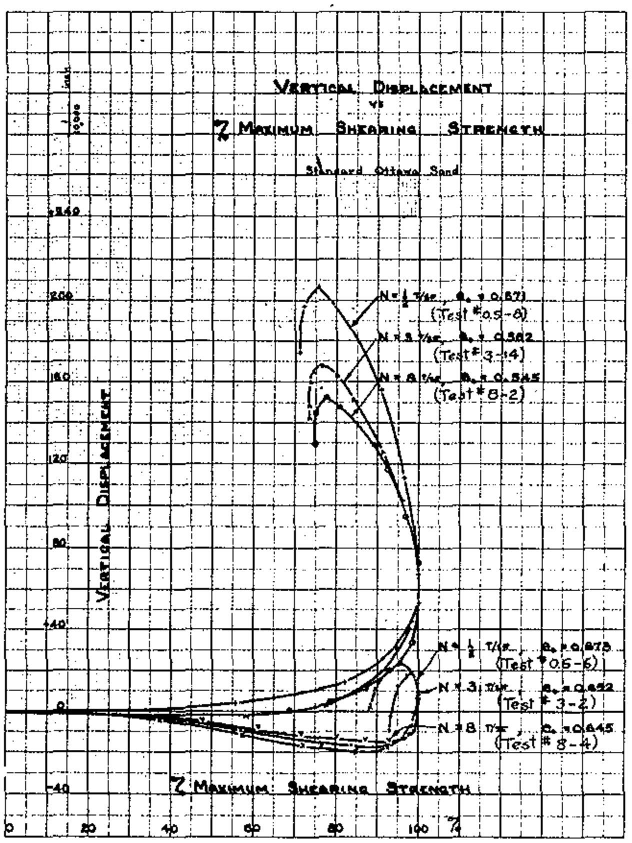

FIG. C-3

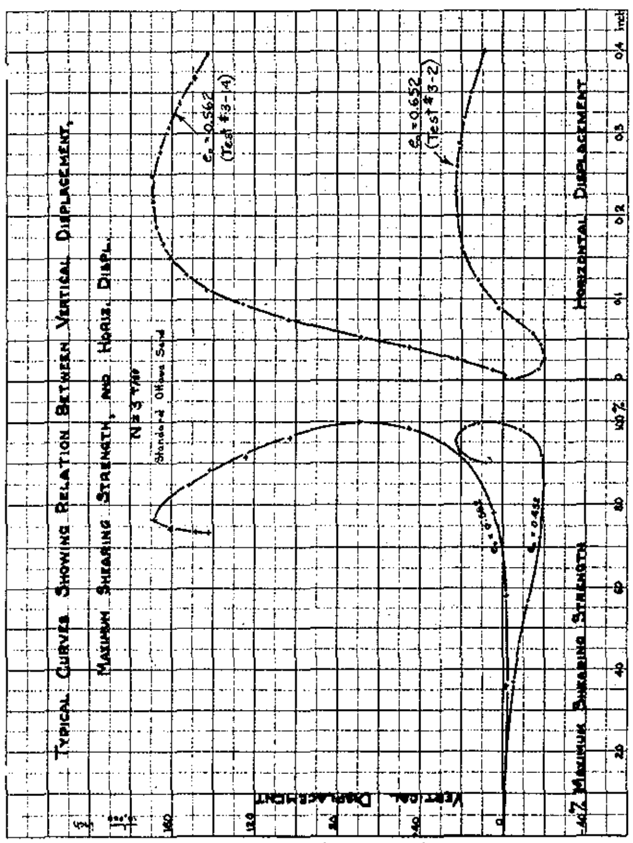

FIG. C-4

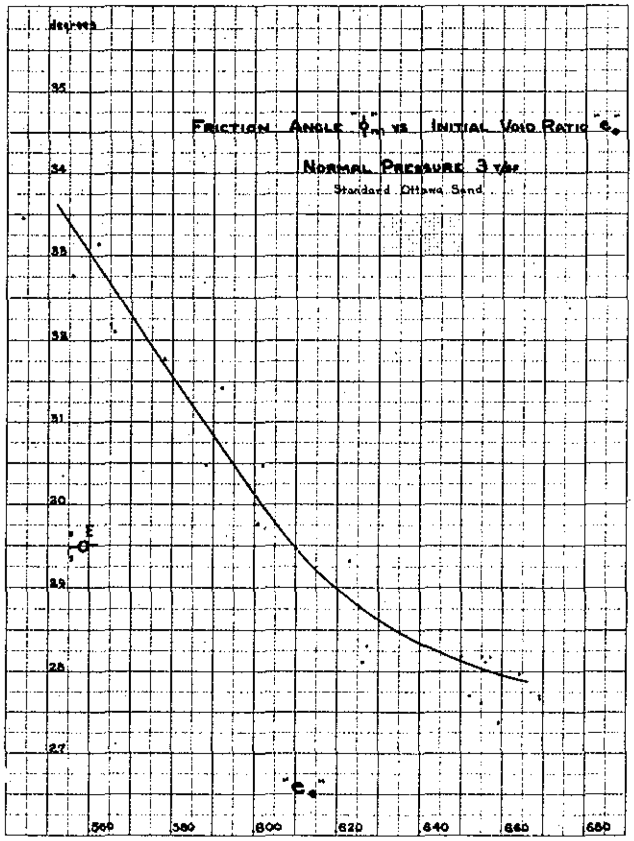

FIG. C-5

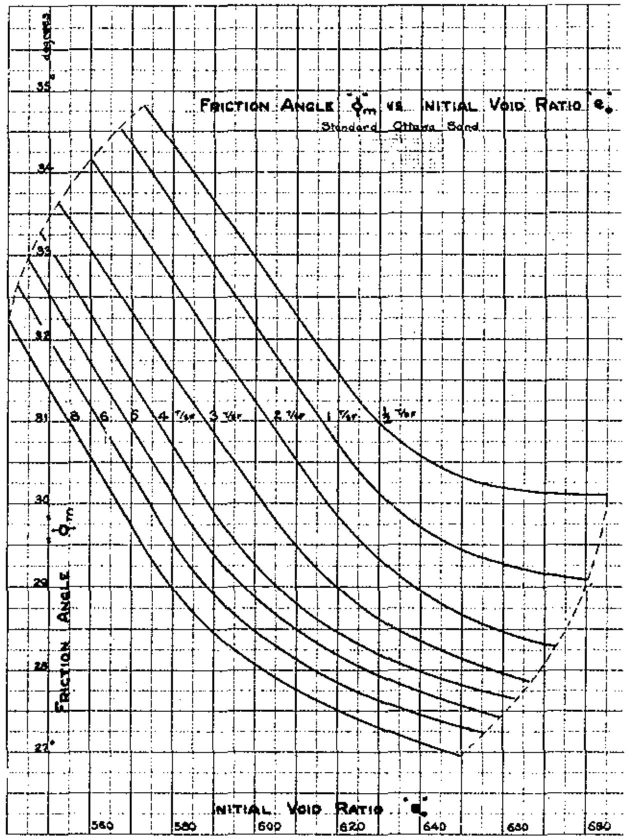

FIG. C-6

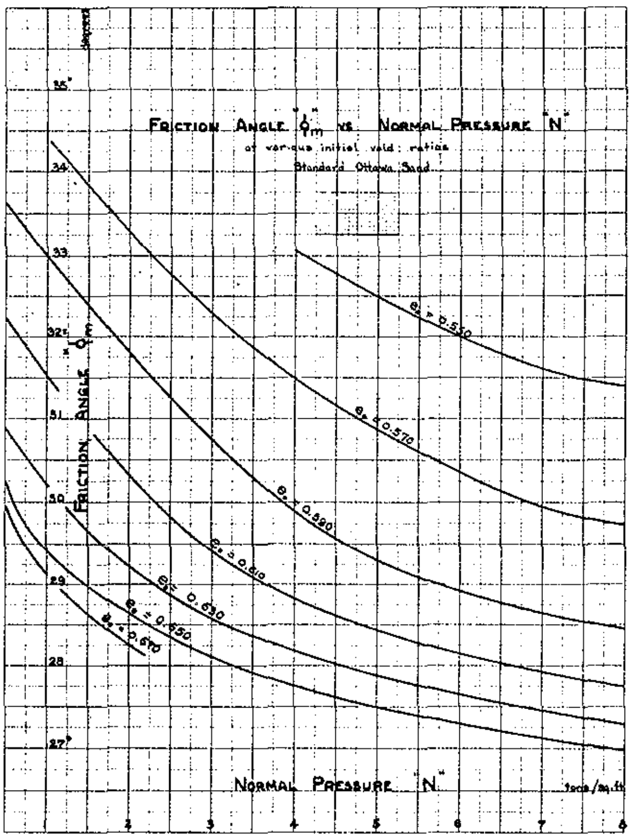

FIG. C-7

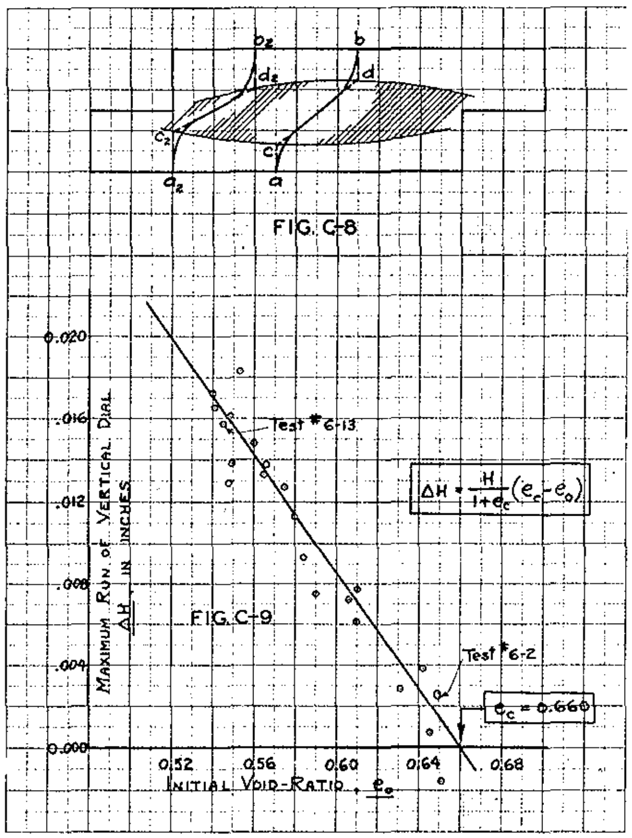

FIG. C-8 & FIG. C-9

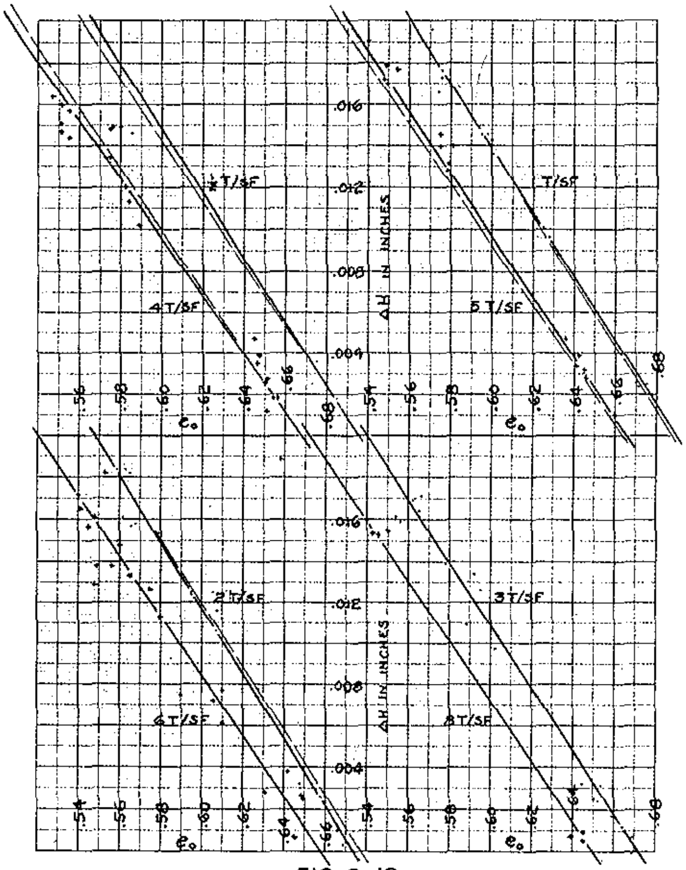

FIG. C-10

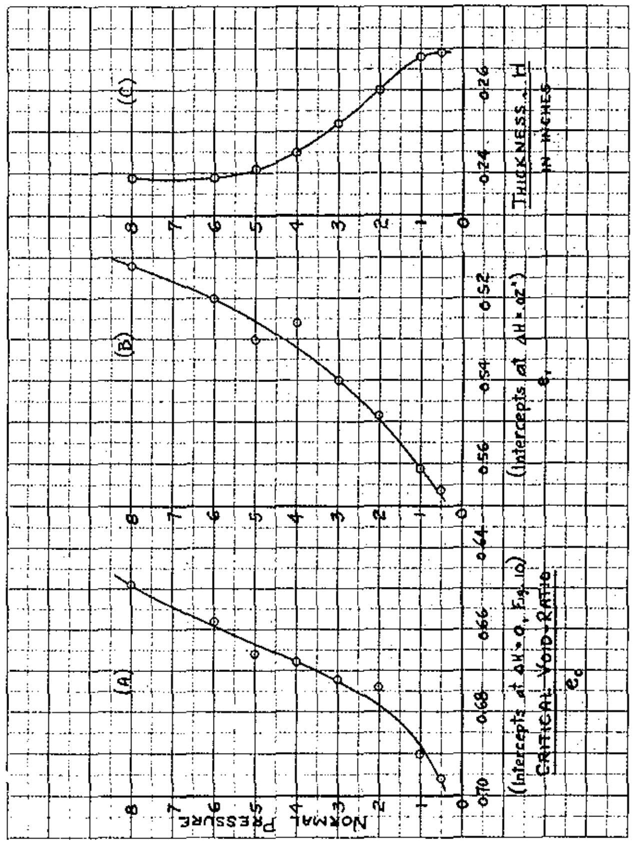

FIG. C-11

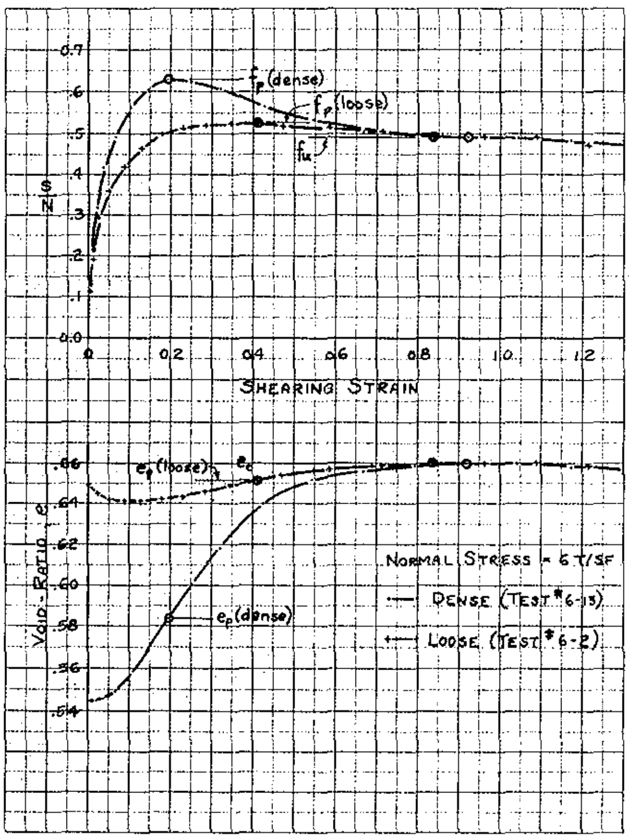

FIG. C-12

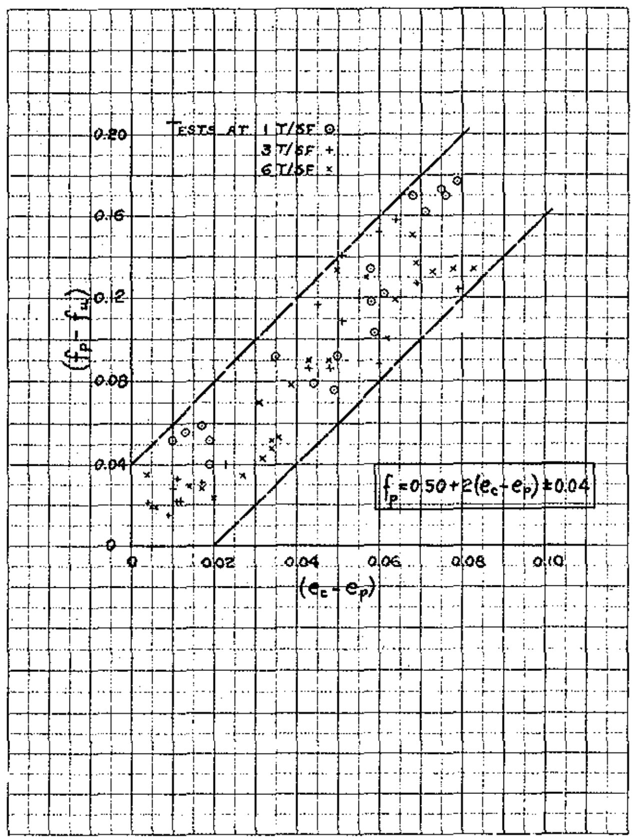

FIG. C-13

A MACHINE FOR DETERMINING THE SHEARING STRENGTH OF SOILS

by

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A MACHINE FOR DETERMINING THE SHEARING STRENGTH OF SOILS

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The majority of problems involving the stability of soil, in an embankment or as a foundation material for example, require a knowledge of the shearing strength of the soil. A number of tests and devices have been used for determining this fundamental property of soil, but most of them have a limited application. At the present time, the type of test commonly used and one that may be applied to all soils is the "direct shear test".

In the direct shear test the soil sample is placed in a box divided into two parts along a horizontal plane. A vertical load is applied to the soil and the horizontal force necessary to produce relative movement between the upper and lower half of the box is determined. This horizontal force divided by the area of the sheared plane is the unit shearing strength of the soil under the conditions tested. The shearing strength of soils increases with an increase in strain up to a certain maximum value. Then in some soils, due to a change in structure, the shearing strength decreases with increased strain and gradually approaches a definite value which is smaller than the maximum. These values are called the maximum and ultimate shearing strengths, respectively.

The variable which has the greatest effect on the shearing strength is the normal pressure on the plane of shear. During shearing tests there are tendencies for changes of volume in the sample and it is essential that these changes be able to occur with absolute freedom and without affecting the vertical pressure on the sample.

The first shearing machine built to allow adequately for vertical expansion and contraction of the sample was designed by Dr. Arthur Casagrande in 1930. In his device, a two-part box holds the soil sample which is 6 cm. square and about 1 cm. thick. A set of two porous stones or toothed gratings grip the sample at the top and bottom. These are used to induce an approximately uniform distribution of shearing stress in the sample. The horizontal load is applied by moving a weight along a beam, which, by means of a rocker causes the upper part of the box to be displaced and thus applies a shearing force to the sample. The load is increased uniformly until failure results; this occurs when the displacement continues without further increase in load. With this type of machine, the maximum shear is found but the ultimate shearing strength cannot be determined.

The first machine applying a uniform rate of movement between the two elements of the shearing box was designed by Dr. Glennon

Gilboy (E.N.R. May 21, 1936, "Improved Soil Testing Methods"). In this machine the vertical load is applied to the specimen by means of a jack reacting against a yoke system and the magnitude of the load is measured by a platform scale on which the shearing box rests. The upper part of the box is connected to a horizontal load-measuring device and the platform scale, with the bottom part of the box rigidly attached to it, is moved horizontally at a uniform rate by means of another jack. At definite intervals, readings are taken of the horizontal force required to hold the upper part of the box stationary, and of the horizontal and vertical movement of one part of the box with reference to the other. By this means the curve of shearing stress vs. shearing displacement may be carried past its maximum to its ultimate value.

On the first few machines of this type, the horizontal load measuring device consisted of a bellows filled with water and registering the pressure by means of a calibrated mercury manometer. A machine of this type (Figure D-1) was designed at the Massachusetts Institute of Technology early in 1936 by Mr. D. W. Taylor, Research Associate in Soil Mechanics. Originally a bellows unit was used as the horizontal load measuring device, but it was not completely satisfactory due to the fact that it compressed as much as 1/2 inch in the total range of load (800 lbs. max. load). This compression of the bellows permitted the vertical yoke to tip slightly which introduced a small undesirable horizontal component of force. Also, the bellows would sometimes kink, a leak would develop in the system or the manometer would break. In addition, it was inconvenient to calibrate the bellows.

To avoid these difficulties, a steel ring, similar to the proving rings used in the calibration of testing machines, was adopted as the load-measuring device. This device has proved to be easy to read, accurate, easily calibrated and it has a small displacement under full load.

A standard proving ring was considered, but it was too elaborate and costly. Professor A. V. de Forest of M.I.T. suggested using a ball bearing race and measuring the deflection (which is a function of the applied load) by means of a dial gauge. Two discarded outer ball bearing races were obtained and reduced to different thicknesses so that one could be used for light loading and the other for heavy loading. They were cut from the inside so that the rings could be interchangeable without the use of shims. A holder was designed to support the ring and hold the measuring gauge in place. By removing two thumb screws, one measuring unit consisting of holder, ring and dial permanently fixed together may be dismounted from the machine and replaced by another. Figure D-2 shows the light and the heavy load measuring units.

Two approximate formulas are needed to calculate the size of ring required to fit the given purpose of deflection and load carrying capacity: $\Delta = .149 \frac{Pr^3}{EI}$ and $f_{max} = .318 \frac{Pry}{I}$, where Δ

is the deflection of the ring along the diameter along which the concentrated load P is applied, r is the radius to the vertical axis of the cross section of the ring which has a moment of inertia of I, a modulus of elasticity of E, a distance of y from the neutral axis to the extreme fiber which has a maximum stress of f_{max}. Any consistent units may be used in the above formulas. The dial gauge can be made to indicate the load directly by having the ring ground to a size where one division on the dial represents a convenient unit of load. The light weight loading ring in the M.I.T. laboratory was designed to have a capacity of 200 pounds with 1/2 pound representing $\frac{1}{10,000}$ inch on the Ames Dial Gauge, whereas one division represents 5 pounds on the heavy loading ring which has a capacity of 1000 pounds.

To minimize permanent set and for strength the rings should be of high carbon steel, heat treated. The rings should be approximately the desired size before heat treating, since they can only be worked with a grinding wheel after treating.

Before calibrating the ring, it should be given its maximum load and then reduced to zero about a dozen times in order to get rid of hysteresis effects. The ring is calibrated by placing it on the platform scale and the loads are applied by means of the vertical loading arrangement of the shearing machine. In calibrating the ring, loads are applied in increments and the dial read. It will be noted that the calibration curve is not quite the same for loading and unloading, but the variation is less than one per cent. In tests, the loading curve is always used because the applied loads increase to a maximum value and then decrease a small amount to the ultimate load. For this small range, however, the difference in the loading and unloading curves is slight.

When the ring has not been used for several days, it should be put through the loading and unloading cycle three times, using about 50% of the capacity load before a test is made. This may be done while it is in place on the machine by pulling against the shear box with the locking pins in place. When this is done it will be found that the deflection of the ring follows the calibration curve exactly. The dials can be read to a fifth of a division since the type of Ames Dial Gauge used has that precision when used over a small range. This corresponds to reading the light ring to 1/10 pound over a range of 200 pounds and reading the heavy ring to one pound over a range of 1000 pounds. Obviously, this accuracy is not claimed or warranted in soil testing, but it indicates that the readings are probably accurate within about one per cent. These rings do not permit a precision equal to that of the standard type of proving rings, but they are direct reading, more convenient to handle and yield sufficiently accurate results for the purpose in hand.

The shearing box (Figure D-3) used in the laboratory at M.I.T. has several novel features. It is made of aluminum with duraluminum posts and screws which saves a great deal of weight, making the final reduction is mounted directly on the jack shaft which also has the hand wheel on it. The worm for this gear is connected to the motor through the flexible shaft so that the worm can be displaced from its gear when hand operation is desired. This worm and gear are so arranged that they can be easily replaced with another set of a different thread.

The 1/30 H.P. motor is of the series wound type so that its speed may be controlled by a rheostat. A 60 to 1 reducer is built in the motor and the worm gear has 80 teeth, single thread. This gear can easily be replaced by one with 80 teeth, 4 threads, so that the maximum speed can be quadrupled. At present, shearing tests may be run to a displacement of 0.4 inch in times of from 5 minutes to an hour.

The framework for the shearing machine is structural steel welded into one solid unit. This is mounted on pipe leg supports at about table height and the whole machine made so that it can be readily moved.

In general, the results by this machine indicate a slightly lower value for the coefficient of internal friction of sands than has been obtained by other machines. This is being further investigated at the present time. Another very important subject now being investigated is the volume change associated with the deformation of the soil. Although it is practically impossible to evaluate this problem quantitatively with the direct shear type of test, it is hoped that many valuable indications to the solution of the problem may be obtained.

Remarkably consistent results have been realized with this apparatus in obtaining the maximum and ultimate shearing resistance of the soils tested. Figures D-5 and D-6 show typical curves obtained by this type of shearing machine. The smooth operation of the machine is one of its chief advantages.

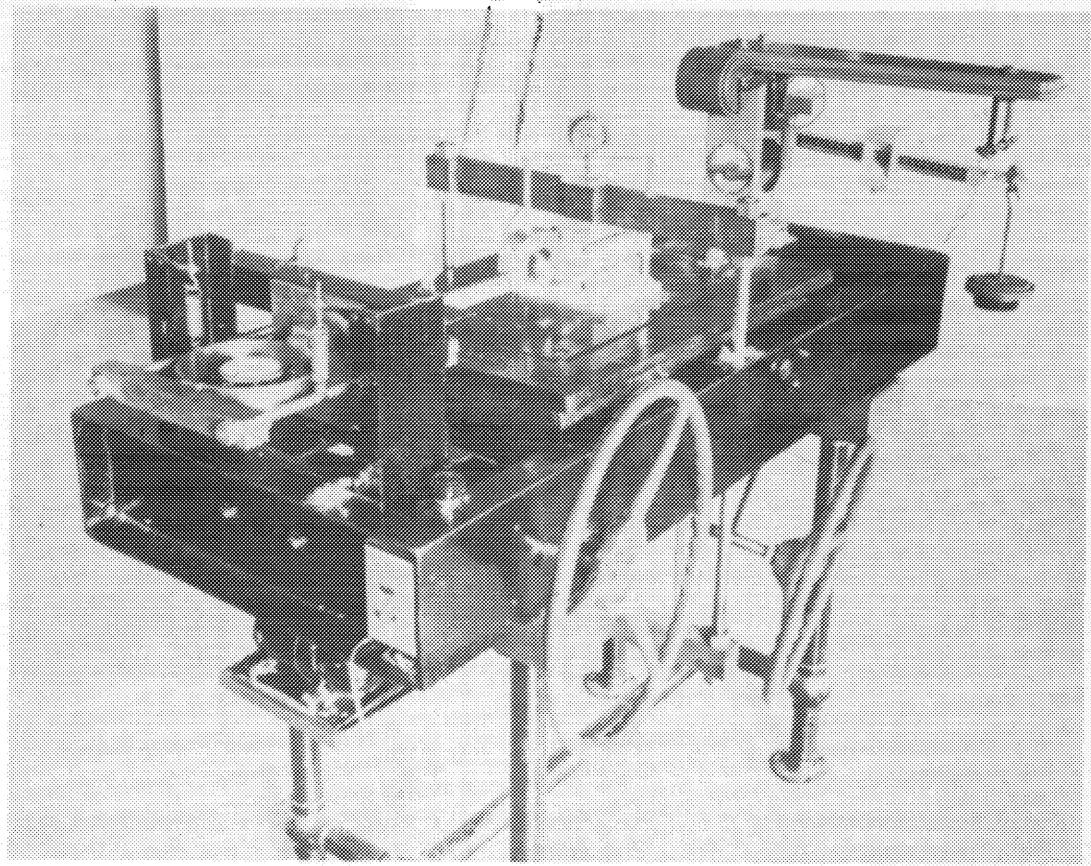

Figure D-1

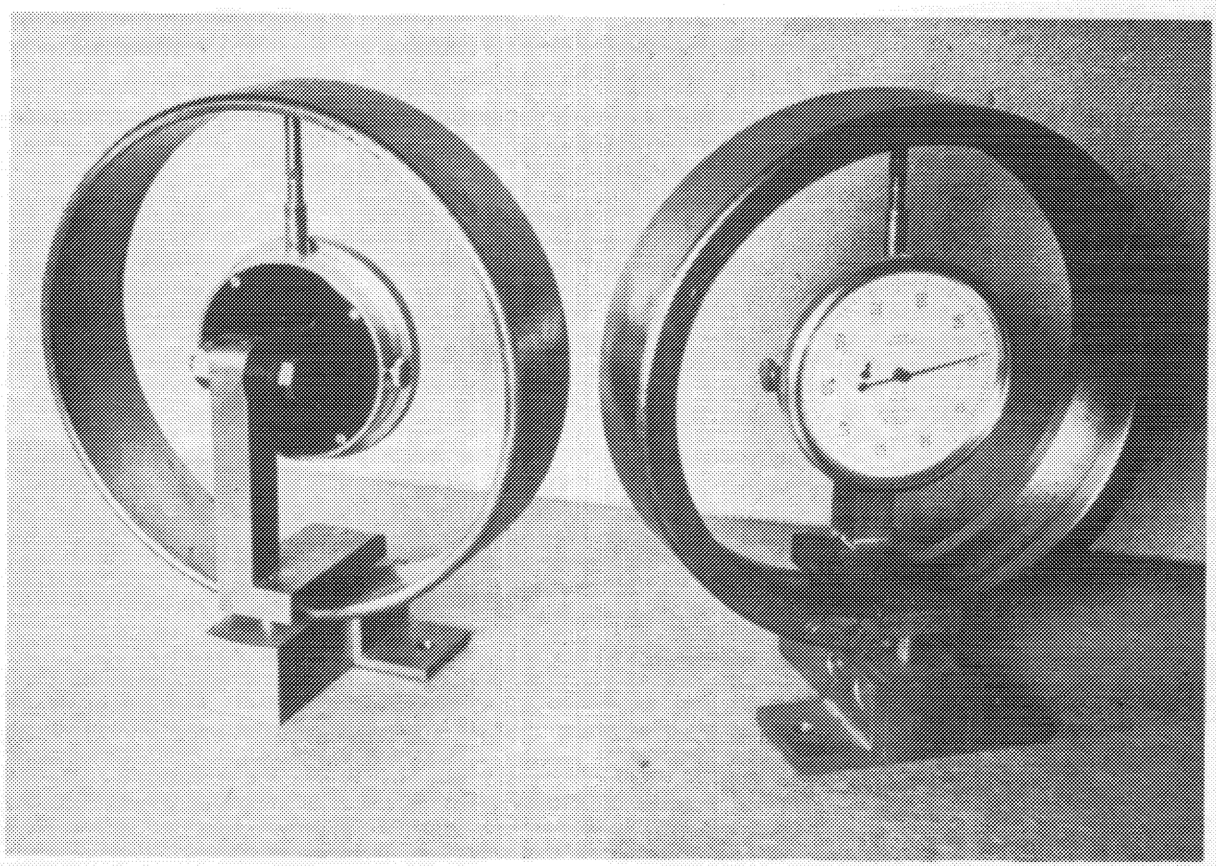

Figure D-2

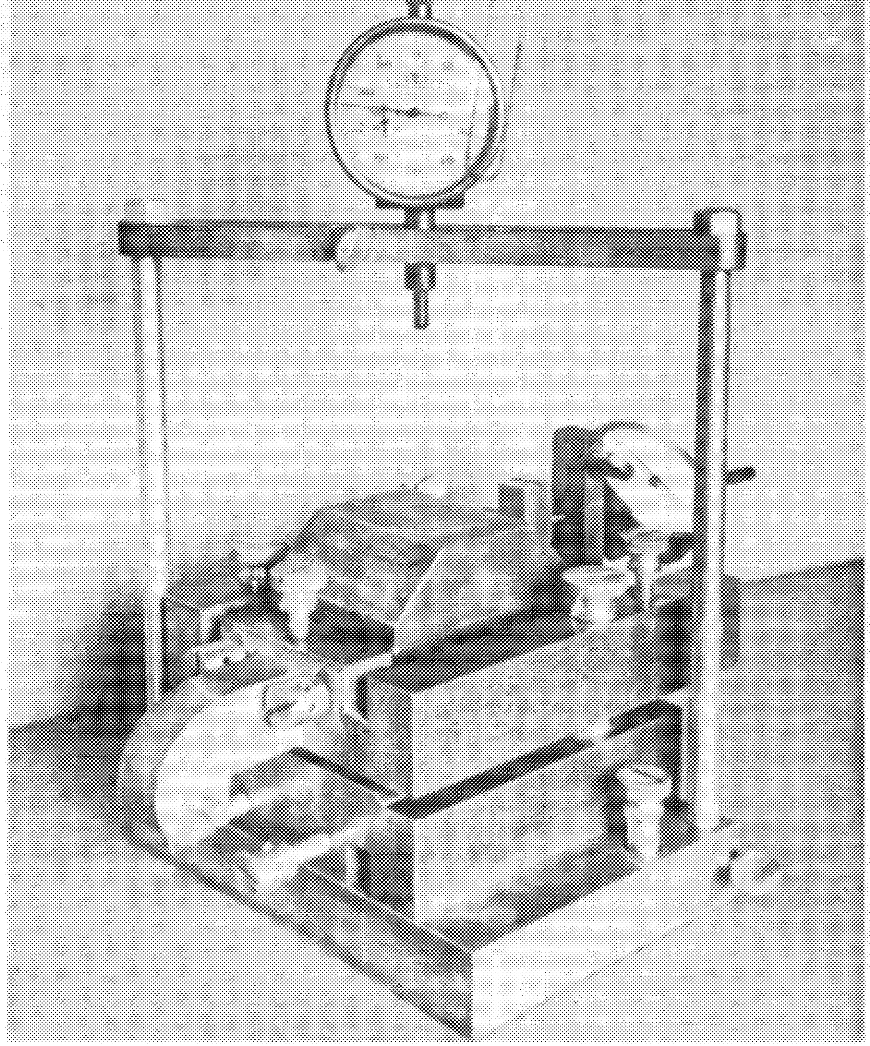

Figure D-3

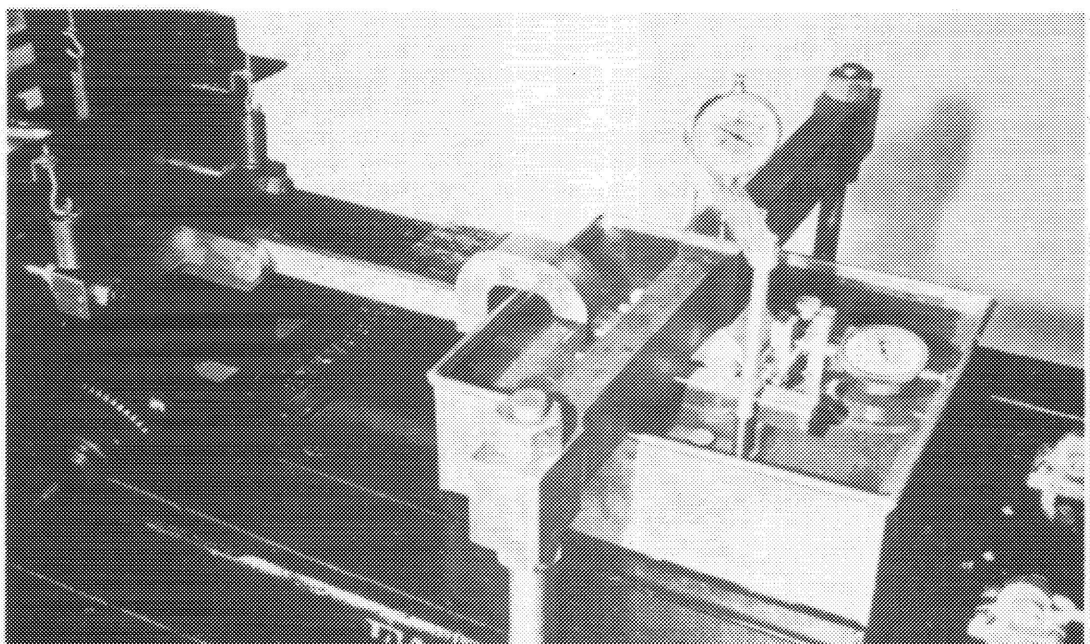

Figure D-4

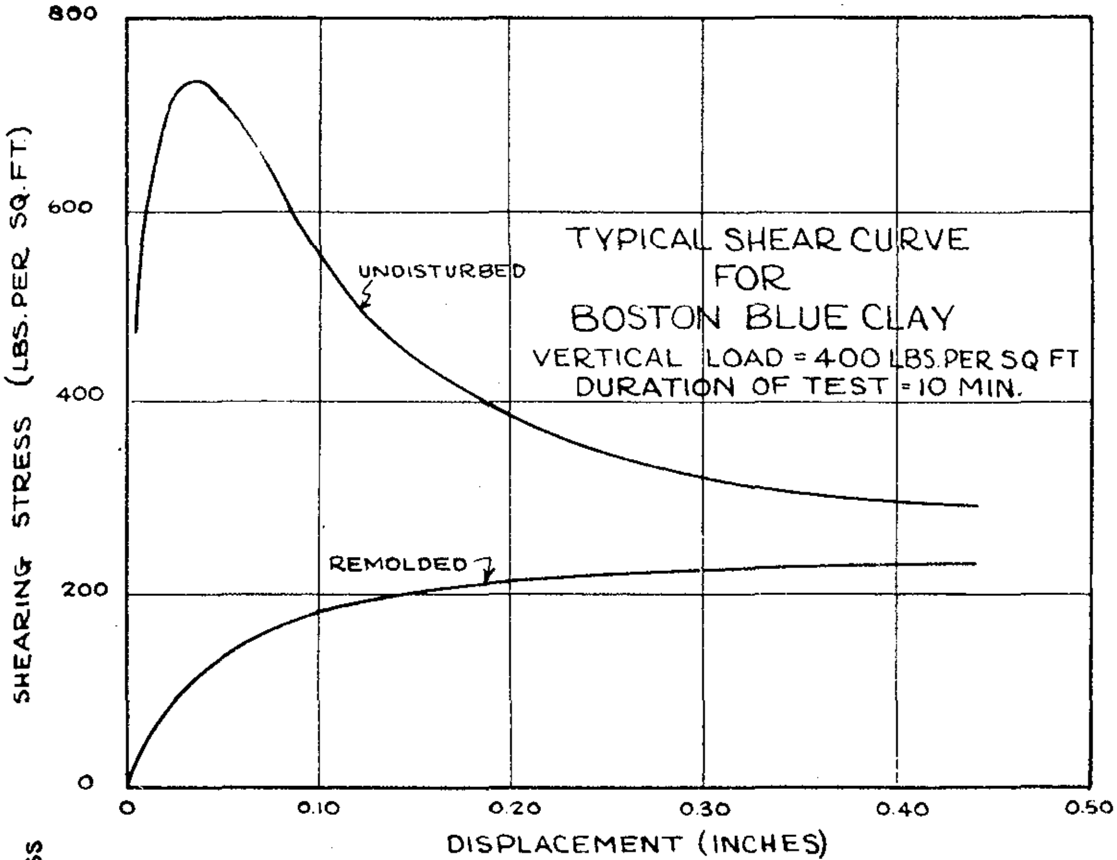

FIGURE **D-5**

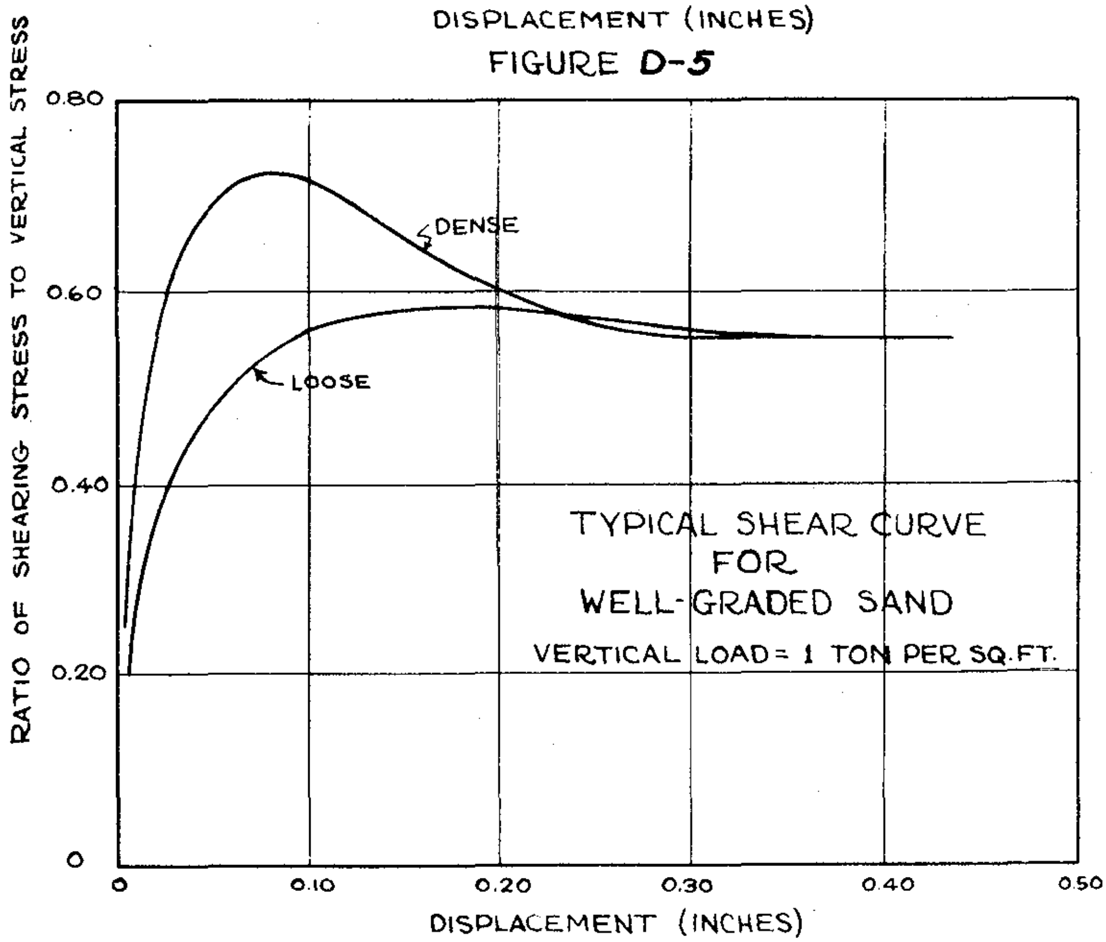

FIGURE **D-6**

THE SHEARING RESISTANCE OF REMOLDED COHESIVE SOILS

by

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for the
American Society of Civil Engineers.

THE SHEARING RESISTANCE OF REMOLDED COHESIVE SOILS

by M. Juul Hvorslev,
Research Engineer for the American Society of Civil Engineers.

INTRODUCTION.

In the years 1934 to 1936 the writer made a series of experiments in Professor Terzaghi's soils mechanics laboratory in Vienna. The object was to find a general expression for the maximum shearing resistance of clays which would be in better agreement with the test results than the commonly used conditions of failure. Some experiments were also made to investigate the slow plastic flow before failure and the decrease of the shearing resistance after failure.

A preliminary account of the results of these tests was published in form of abstracts in the Proceedings of the International Conference on Soil Mechanics and Foundation Engineering at Harvard University in 1936. The complete report on the test results and the final conclusions drawn therefrom were submitted as a thesis to the Vienna Institute of Technology. According to the requirements, this report was written in and later also published in German. As the above-mentioned abstracts are too abbreviated to be readily understood, a more detailed account of some of the results of these tests will be presented here.

In order to obtain consistent results large amounts of absolutely uniform testing material are required for such investigations and it becomes necessary to use remolded clay. The physical properties of undisturbed clays are not sufficiently uniform and it is difficult to avoid a partial disturbance of the soil structure during the preparation of test specimens. Furthermore, the undisturbed clay has a definite stress history which may not suit the particular test. The results of tests on remolded clays may not be directly applicable to undisturbed clays, but such tests are a necessary preliminary step towards a general understanding of the elastic properties and strength of both remolded and undisturbed cohesive soils.

NOTATION.

In order to facilitate the use of the original drawings for the aforementioned publication in German, the majority of the symbols used therein will be retained, although in a few instances they differ from the symbols commonly used in this country. A general list and explanation of these symbols are given below.

B	Index of virgin compression.
c	Cohesion.
e	Basis of natural (napierian) logarithms.

H	Thickness of sample.
ΔH	Change in thickness of sample.
ln	Natural or napierian logarithms ($\log_e$).
n	Porosity.
n_c	Degree of overconsolidation.
p	Effective consolidation pressure.
p	Effective normal stress on plane of shear failure.
p_1	Unit consolidation pressure ($p_1 = 1$).
p_e	Equivalent consolidation pressure.
p_k	Capillary pressure.
p_m	Pre-consolidation pressure.
q	Compressive strength.
s	Shearing resistance (τ_{max}).
T_s	Equivalent duration of shearing test.
u	Hydrostatic pressure in the porewater.
w	Moisture content.
w_i	Plasticity index.
w_l	Liquid limit.
w_p	Plastic limit.
w_s	Shrinkage limit.
α	Angle between the first principal stress and the plane of failure.
β	Angle between first principal stress and plane of stratification.
ε	Void ratio.
ε_1	Void ratio for $p = p_1$.
$\varkappa$	Coefficient of effective cohesion.
μ	Coefficient of internal friction.
μ_o	Coefficient of effective internal friction.
μ_c	Krey-Tiedemann coefficient of cohesion.
μ_r	Krey-Tiedemann coefficient of internal friction.
μ_s	Coefficient of apparent internal friction.
ν	Specific coefficient of effective cohesion.
σ	Effective normal stress.
σ_t	Total normal stress.
τ	Effective shearing stress.
τ_t	Total shearing stress.
ϕ	Angle of internal friction (suffixes as for u).

IMPORTANT PROPERTIES OF COHESIVE SOILS

Soils in general are polyphase systems with mineral grains as the dispersed or solid phase and with air and water as the dispersing media. In the following, however, only fully saturated soils will be considered. As indicated by its name, the main property of a cohesive soil is the possession of the so-called true cohesion in contrast to the apparent cohesion, caused by capillary and flow pressures, which also appears in cohesionless soils. Furthermore, the cohesive soils are generally able to undergo plastic deformations. According to current conceptions, these properties are mainly due to the content of minute particles of certain minerals, the so-called clay minerals. Due to their crystal structure these particles have the form of flakes and are able to attract and orient the bi-polar water molecules in such a manner that the water changes its original properties; the density and the viscosity increases the closer we come to the surface of the flaky particles. The particles may be said to be surrounded by films of partly solidified water. When these films overlap, certain internal forces are created and bind the particles together. We do not yet know the exact nature of these water films and the forces they create, and the above conception is just a rough working hypothesis.

The structure of an undisturbed cohesive soil is generally more or less stratified, but after a thorough remolding we may assume that even the flaky mineral particles have no preferred orientation. However, when such a remolded clay is reconsolidated, the flaky mineral particles will orient themselves in planes perpendicular to the direction of the major principal stress and we obtain an anisotropic material. This must be taken into consideration in the planning and interpretation of laboratory experiments.

The existence of such a stratification in clays can be demonstrated in several ways, for example, by the following simple test. From a remolded sample of clay, consolidated by confined compression and then partially air-dried, thin slices are cut out parallel and perpendicular, respectively, to the direction of the principal consolidating force. These slices are then laid in water and, as will be seen from Fig. E-1A and E-1B, cracks occur at right angle to the direction of the major principal stress. If, however, a ball of remolded clay is formed and only air-dried, then the consolidating force - capillary pressure - will be of equal magnitude in all directions and cracks in a thin slice, cut from the ball and laid in water, will not have any preferred orientation.

Many undisturbed soils become soft when they are kneaded; this decrease in strength is primarily due to the disturbance of a complex internal structure, built up of grains of various sizes and flocs of small grains (Casagrande (2)).* However, many remolded soils also

* This reference number in parentheses, as well as others which follow in the text, refers to the numbered bibliography on pages E-28 to E-30.

soften when they are kneaded some time after the first remolding, but this decrease in strength is more or less reversible. After the molding stops the clay will regain its strength partly or entirely in course of time. This property is called thixotropy, and the loss of strength can be explained roughly as a temporary disturbance of the structure of the films of water surrounding the clay minerals (Freundlich (6)). Closely connected with the thixotropic phenomena is the variable viscosity of clays undergoing plastic deformations. This so-called structural viscosity varies with the disturbance that has taken place. The effect of these two properties on the interpretation of the results of shearing tests will be discussed later.

Of primary importance for the determination of the deformations and shearing resistance of soils is the distribution of the stresses between the solid and liquid phase of the soil.

When a soil is subjected to stress, one part of this stress will be transmitted through the surfaces of contact between the mineral grains or between the films of water surrounding these grains and another part of the stress will be carried by the free water in the pores. Is σ_t the total normal stress on a certain plane, σ the normal stress carried by the mineral grains, also called the particle or effective stress, and u the hydrostatic pressure in the porewater, then

$$\sigma_t = \sigma + u . \tag{1}$$

As the value of u at a certain point is independent of the direction of the plane under consideration, the total shearing stress τ_t must be equal to the effective shearing stress τ .

$$\tau_t = \tau . \tag{2}$$

That is, the water does not transmit any part of the shearing stresses. From the work of Terzaghi, Casagrande, Jürgenson, Rendulic, and others we know that it is solely the effective stresses which govern the conditions of failure and the ultimate deformations of soils. The value of the hydrostatic pressure in the porewater is of no importance when we operate with effective stresses. However, in most stress computations the total stresses are determined first, and in this case the hydrostatic pressure in the porewater is of extreme importance for the conditions of failure. It is often very difficult to determine the effective stresses and therefore tempting to operate with the total stresses, but, in many instances, this procedure has been the cause of misinterpretation of shearing tests on saturated soils.

A value of u equal to a hydrostatic pressure corresponding to the depth under the free water surface is called the natural hydrostatic pressure in the porewater. The difference between the actual and natural hydrostatic pressures is called the excess pressure, which may be either positive or negative. In case the soil is acted upon by a capillary pressure, p_k, the hydrostatic pressure in the porewater right below the surface has the numerical value p_k but is negative, or $u = - p_k$. In case of flow in the porewater a flow pressure corresponding to the slope of the hydraulic gradient is transmitted to the mineral grains through friction and is thereby added directly to the effective stresses.

If the stress conditions are changed the soil will be subject to considerable changes in volume or, as we generally express it, the void ratio will change. Furthermore, the volume or void ratio depends not only on the actual stress condition but on the whole stress history of the soil. In Figure E-4 is shown a typical pressure-void ratio-diagram for a remolded soil reconsolidated by confined compression. The void ratio, ε, is plotted as ordinates and the effective vertical stresses or consolidation pressure, p, as abscissae. The curves on the right are identical with those on the left, but here the logarithms of p are plotted as abscissae. The curve o-m-b we call the virgin consolidation curve, m-a the rebound or expansion curve, and a-b the recompression curve. The corresponding states of consolidation are called natural consolidation, simple overconsolidation, and cyclic overconsolidation. The effective vertical pressure p_m corresponding to the start of the rebound curve is called the pre-consolidation pressure.

The virgin consolidation curve can be represented approximately by the following equation, in its basic form first given by Terzaghi:

$$\varepsilon = - \frac{1}{B} \ln \frac{p}{p_1} + \varepsilon_1 \qquad (3)$$

where ε is the void ratio, p the actual, effective consolidation pressure, p_1 the unit of pressure ($p_1 = 1$); B is a dimensionless constant called the index of virgin compression and equal to the slope of the line o-m-b in the semi-logarithmic plot when natural logarithms are used; ε_1 is the void ratio corresponding to $p = p_1$. The void ratio ε_1 varies to some extent with the void ratio at the start of the test and the manner in which the test is carried out, while B or the slope of the line o-m-b in Figure E-4B remains fairly constant; that is, the curves as a whole may be shifted a little to the right or left according to the test conditions.

In order to facilitate the analytical expression and graphical representation of the influence of the void ratio on the shearing resistance and compressive strength, the equivalent consolidation pressure or, in short, the equivalent pressure will be used later and is defined as the pressure in the virgin pressure-void ratio-diagram which corresponds to the actual void ratio of the soil. From equation (3) we obtain,

$$p_e = p_1 \quad e^{B(\mathcal{E}_1 - \mathcal{E})}, \tag{4}$$

where e is the basis of natural or napierian logarithms. The ratio n_c between the equivalent pressure p_e and the actual pressure p,

$$n_c = \frac{p_e}{p} \quad , \tag{5}$$

we will call the degree of overconsolidation. Graphical representation of compressive strength by means of equivalent pressures were first used by Terzaghi and Janiczek (page 560 in (19)).

COMMONLY USED CONDITIONS OF FAILURE.

When the shearing load is increased in steps and not continuously, we observe after each increase in shearing stress that the velocity of deformation decreases with time until it finally becomes zero. In other words, there is a definite relationship between stress and strain. However, as the shearing load increases the time required to bring the displacements to a complete stop also increases. Finally we reach a shearing stress at which the velocity of the displacements does not decrease to zero but approaches a certain definite, very small value. A continuous, slow plastic flow is taking place. Terzaghi has called the shearing stress at which this slow plastic flow starts the bond resistance (see (18)). If the shearing stresses are further increased both the initial and ultimate velocities of displacement after each load increment also increase. There is no longer a definite relationship between stress and strain but only between the stress and the velocity of the slow plastic flow. When we get close to the maximum shearing resistance there may be sudden increases in the velocity of displacement, but after each of these spurts the velocity again decreases to the velocity of the slow plastic flow. On the other hand, when a certain higher shearing stress is reached the velocity of displacement keeps on increasing and in order to effect a decrease of the velocity we must decrease the shearing load. This maximum shearing stress is called the shearing resistance.

Several investigators have used the shearing stress at which the first sudden increase in the displacements occur as the shearing resistance. This may be sound from a practical standpoint, but this shearing stress may vary, in case of identical tests, between 85 and 100% of the maximum shearing resistance, which is a much more definite value. In the following we shall therefore use the actual maximum value of the shearing stress as the shearing resistance.

The shearing resistance is composed of two parts, a frictional resistance and a cohesion. As conceived in the following, the cohesion is not necessarily a constant for a given material but is only constant for a given point in the soil under given stress conditions; that is, the cohesion has the same value for all planes through that point, while the frictional resistance varies with the normal effective stress on the plane under consideration.

The oldest and most commonly used expression for the maximum shearing resistance of cohesive soils is given by Coulomb as follows:

$$s = \mu p + c = p \tan \phi + c , \tag{6}$$

where s is the shearing resistance and p the effective normal stress on the plane of failure; μ is the coefficient and ϕ the angle of internal friction; c is the cohesion. This relationship is graphically represented by the line a-b in Figure E-5. We call this line the shearing resistance curve. When ϕ and c are constant or independent of the stress conditions and the direction of the planes of failure we have the well-known relation

$$\alpha = 45 - \frac{\phi}{2} , \tag{7}$$

where α is the angle between the direction of the major principal stress and the plane of failure, Figure E-6.

If the cohesion is zero, we obtain the shearing resistance curve o-m or

$$s = \mu_s p = p \tan \phi_s . \tag{8}$$

This expression applies in general to cohesionless materials, but in case of cohesive soils in a state of natural consolidation the shearing resistance curve will often be a straight line through origo. In this case, however, the angle ϕ_s does not represent the true angle of internal friction, as we, by changing the state of natural consolidation to one of overconsolidation, can obtain shearing resistance curves corresponding to the line a-b and thereby another angle ϕ ; that is, for the point of intersection between the lines a-b and o-m we have the choice between two angles of internal friction, while the angles ϕ determined by means of the inclination of the planes of failure and equation (7) seem to be independent of the state of consolidation of the soil. Therefore, when applied to cohesive soils we call the angle φ_s the angle of apparent internal friction.

By means of Mohr's stress diagram, shown in Figure E-7, we can express the condition of failure in terms of the major and minor effective principal stresses, σ_1 and σ_3, as follows:

$$\sigma_1 - \sigma_3 = 2 \sin\varphi (p_i + \tfrac{1}{2} (\sigma_1 + \sigma_3)) , \qquad (9)$$

where

$$p_i = \frac{c}{\tan \varphi} \qquad (10)$$

indicates the magnitude of the internal forces created by the overlapping of the waterfilms surrounding the clay minerals. In case c and thereby p_i is zero, we obtain

$$\frac{\sigma_1 - \sigma_3}{\sigma_1 + \sigma_3} = \sin \varphi_s . \qquad (11)$$

Equation (10) is known as Mohr's condition of failure and equation (11) as Rankine's condition of failure, but both are just other expressions for Coulomb's law.

During the first years of experimentation in modern soil mechanics laboratories it was found that both φ and c varied between rather wide limits, and the whole subject of the actual shearing resistance of cohesive soils became confused and uncertain. This confusion was due largely to the fact that the influence of capillary and flow pressures was not yet realized and that, in many of the earlier shearing tests, the porewater carried a part of the normal stresses on the plane of failure. Through the experiments by Casagrande (5) and Jürgenson (10) it was shown that the soils, in contrast to other building materials, undergo considerable volume changes when subjected to pure shear. Consequently, an increase in the shearing stresses creates a temporary excess pressure in the porewater and the effective normal stress is then not any more equal to the external pressure. In order to obtain effective normal stresses equal to the external pressure, the shearing test must be extended over a period long enough to allow equalization of the excess pressures in the porewater.

Even in case of such slow shearing tests we can obtain any number of shearing resistance curves for a given soil. Krey (12) experimented with clays in a state of natural and simple overconsolidation and determined shearing resistance curves for various pre-consolidation pressures. Tiedemann (see page 37 in (16) and page 114 in (1)) extended these experiments, observed that these shearing resistance curves were nearly straight and parallel, and suggested that the cohesion was directly proportional to the pre-consolidation pressure p_m, thereby obtaining the following condition of failure:

$$s = \mu_r p + \mu_c p_m = p \tan \varphi_r + p_m \tan \varphi_c . \quad (12)$$

The graphical determination of the coefficients μ_r and μ_c and the angles φ_r and φ_c are shown in Figure E-8, where the line $o-m_1-m$ is the shearing resistance curve for the material in the state of natural consolidation. In the latter case we have $p_m = p$ and $c = \mu_c p$ or

$$s = p \tan \varphi_s = p \tan \varphi_r + c . \quad (13)$$

By this interpretation of the results of the shearing tests we obtain the same angle of internal friction φ_r whether the soil is in a state of natural consolidation or overconsolidation and the variations of the cohesion are thereby partly accounted for.

Although the Krey-Tiedemann condition of failure is a decided improvement over the old Coulomb condition of failure it does not fully explain all the test results. The angles φ_r are still considerably larger than those obtained by means of simple compression tests and equation (7). Furthermore, the shearing resistance curves for overconsolidated clays are actually not straight lines but curves and form a complete hysteresis loop, Figure E-9, when the experiments are extended to include cyclic overconsolidated soils (Terzaghi (17) and page 550 in (19)). This means that we can obtain two different values for the shearing resistance for the same pre-consolidation pressure, p_m, and the same effective normal stress, p.

A NEW CONDITION OF FAILURE.

The above-mentioned shortcomings of the Krey-Tiedemann condition of failure and the similarity between the shearing resistance hysteresis loop and the pressure-void ratio-diagram led the writer to believe that the cohesion mainly is a function of the void ratio, or of the already defined equivalent consolidation pressure. The investigation of this hypothesis was one of the main objects of the experiments.

The tests were carried out with two entirely different clays, the silty Vienna Clay and the fat, highly plastic Little Belt Clay; both are marine, tertiary clays. The grain-size distribution curves and the Atterberg limits are shown in Figure E-10. Vienna Clay contains about 35% of particles smaller than 0.005 mm, has a liquid limit of 46% and a plastic limit of 22%, while Little Belt Clay contains 93% of particles smaller than 0.005 mm, has a liquid limit of 128% and a plastic limit of 36%. The pressure-void ratio-diagrams for the two soils are shown in Figures E-11 and E-12. As mentioned before, the testing procedure may have some influence on the location of the curves but comparatively little influence on their slope. The dotted lines represent curves of virgin consolidation, determined by confined compression in the shearing boxes under exactly the same loading procedure which was used in the preparation of the shearing test specimens; these curves were therefore used in the determination of the equivalent consolidation pressures.

The stock material of the two soils was thoroughly remolded and mixed with distilled water until a moisture content close to the liquid limits for the respective soils was reached. From this stock material natural consolidated, simple and cyclic overconsolidated shearing tests specimens were prepared. The pre-consolidation pressure was 2.0 or 5.0 kg/cm^2. One week was allowed for the initial consolidation and one week each for rebound and re-consolidation; the preparation of a cyclic overconsolidated test specimen thereby required about three weeks.

The shearing resistance was determined by means of the Krey shearing apparatus with Terzaghi shearing boxes, shown in Figure E-13. Special arrangements were made to measure the vertical movements of the surface of the sample and of the upper part of the shearing boxes. Several months were used for investigation of the influence of friction and of the displacements and vertical movements of various parts of the shearing apparatus and shearing boxes. The shearing loads were applied in increments of about 2% of the maximum shearing load and at time intervals which varied from 2 minutes at the start of the test to 30 or 45 minutes towards the end of the test. The total duration of a single shearing test varied from 8 to 16 hours, according to the material and the state of consolidation of the sample. Extensive preliminary tests had established that this was sufficient time to allow a practically complete equalization of the excess pressures in the porewater, created by the shearing stresses. The vertical normal stresses corresponding to the external normal load were therefore equal to the effective normal stresses on the plane of failure. The shearing boxes were removed from the shearing apparatus and the average moisture content in the zone of failure, Figure E-14, determined as rapidly as possible after the maximum shearing stress was reached.

The results of the tests are shown in Figures E-15 and E-16. The full drawn lines in the top sections represent the moisture contents at the moment of failure. The full drawn lines in the center sections are the shearing resistance curves, O-A for natural consolidation, A-B for simple overconsolidation, and B-C for cyclic overconsolidation. The heavy dotted lines represent the corresponding equivalent consolidation pressures, p_e, at the moment of failure, while the thin, dash-dot lines show the same pressures at the start of the shearing test. The latter curves were plotted from consolidometer tests (Figures E-11 and E-12), and the difference in consolidation procedure must be taken into account. In the lower sections are plotted the total vertical deformations of the sample.

The last-mentioned curves show that natural or slightly overconsolidated clays suffer a volume decrease during the shearing, while strongly overconsolidated clays are subject to an increase in volume. This means that a sudden increase in the shearing stress will call forth either a positive or negative excess pressure in the porewater according to the state of consolidation of the soil. However, at certain definite states of overconsolidation the shearing stresses will not cause any volume changes or any excess pressures in the porewater. These states of overconsolidation correspond to the critical void ratio of cohesionless materials, defined by Casagrande (3), and we can say that in case of cohesive soils any void ratio can become critical if it is produced by a critical consolidation procedure. These facts are very important in the interpretation of the results of rapid shearing tests, which will be discussed later.

We now observe that the curves for the equivalent consolidation pressures at the moment of failure are nearly identical in shape to the shearing resistance curves. Assuming that the coefficient of friction is a constant, this similarity suggests a linear relation between the cohesion and the equivalent consolidation pressure at the moment of failure or the following condition of failure:

$$s = \mu_o \, p + \varkappa \, p_e \, , \tag{14}$$

where μ_o is the coefficient of effective internal friction and $\varkappa$ the coefficient of effective cohesion. Equation (14) can also be written in the following form:

$$\frac{s}{p_e} = \mu_o \cdot \frac{p}{p_e} + \varkappa = \frac{p}{p_e} \tan \varphi_o + \varkappa \tag{15}$$

In this manner we have obtained a condition of failure of the same simple form as the Coulomb condition of failure. If the proposed condition of failure is correct, the points $\left(\frac{p}{p_e} , \frac{s}{p_e} \right)$ must lie on a straight line, making the angle φ_o with the abscissae axis and intersecting the ordinate axis at $\partial\ell$. Figure E-17 shows the test results plotted in this manner, and it will be seen that in case of Vienna Clay the points lie nearly exactly on a straight line; this was also the case for a second series of tests with Vienna Clay, the results of which are shown in Figure E-23. In case of Little Belt Clay, however, the points are somewhat scattered, especially in case of points corresponding to the shearing resistance of samples in a state of natural consolidation. Nevertheless, the approximation seems very satisfactory when we consider that the Coulomb or Krey-Tiedemann condition of failure could not very well be applied to Little Belt Clay, as all the shearing resistance curves are decidedly curved. By plotting the test results in this manner we have, furthermore, replaced the shearing resistance hysteresis loops with a single straight line, thereby providing an easy means of averaging all the test results.

By means of the relation between p_e and $\mathcal{E}$ - equation (4) - and by introducing the coefficient

$$\mathcal{V} = \partial\ell \, p_1 \; e^{B\,\mathcal{E}_1} , \tag{16}$$

equation (13) takes the following form:

$$s = \mu_o \; p + \mathcal{V} \, e^{-B\mathcal{E}} \tag{17}$$

or, expressed in words, the shearing resistance is a function of the effective normal stress on and the void ratio in the plane of failure and at the moment of failure, and this function is independent of the stress history of the soil.

The proposed condition of failure takes into consideration the effect of volume changes, not only during the initial consolidation but also during the rebound and recompression and during the shearing test. The angles ϕ_o are considerably smaller than the angles ϕ_r obtained by the Krey-Tiedemann condition of failure - see Figure E-23. This difference is principally due to the circumstance that the Krey-Tiedemann condition of failure does not take the volume decrease and corresponding increase in cohesion during the shearing tests on soils in a state of natural consolidation into consideration. In cases where the shearing resistance line for the state of natural consolidation is straight - Vienna Clay - the increase in cohesion, Δc, during the shearing test on a sample in the state of natural consolidation can be expressed as follows:

$$\Delta c = \partial\ell \; (p_e - p) = \partial\ell \, (n_c - 1)p , \tag{18}$$

where $n_c = \frac{p_e}{p}$ is a constant for the particular soil. The corresponding change in void ratio, $\Delta \varepsilon$ we obtain by means of equation (4).

$$\Delta \varepsilon = - \frac{\ln n_c}{B} . \tag{19}$$

In case of Vienna Clay we have $n_c = 1.72$, or an increase in the cohesion during the shearing test of 72% of the cohesion at the start of the test.

The proposed condition of failure applies only to saturated cohesive soils and should be considered only as an approximation of the actual conditions, as we have assumed that the coefficient of effective internal friction is a constant and as the cohesion, besides being a function of the void ratio, probably also is dependent on the ratio between the free and "partly solidified" water in the pores.

COMPRESSIVE STRENGTH AND INFLUENCE OF STRATIFICATION.

It now remains to be investigated if the angles ϕ_o, determined by means of the proposed condition of failure, are in agreement with the angles of internal friction obtained by means of simple unconfined compression tests and measurement of the angles of inclination of the planes of failure. Furthermore, it should be determined if the direction of the principal orientation of the flaky particles in the clay has any influence on the friction and cohesion. It must be remembered that in case of shearing tests on remolded, reconsolidated clay the general direction of the orientation of these flaky particles is parallel to the plane of failure.

In order to make the above-mentioned investigations three series of simple unconfined compression tests were carried out. The test specimens were cut out of a large sample, consolidated under 5.0 kg/cm^2 in such a manner that the angle β between the axis of the test specimen and the planes of orientation of the flaky particles was 0^o, 45^o and 90^o, respectively, Type III, II and I in Figure E-18. The cross-section of the test specimens was a square, as this facilitated their preparation and also the measurement of the angles of inclination of the planes of failure. Comparative tests with cylindrical tests specimens were also made; they proved to have about 8% greater strength than the prismatic specimens, but the angles of inclination of the planes of failure were nearly equal in the two cases.

The compressive strength q is a function of the friction and cohesion in the plane of failure and of the capillary pressure p_k.

At the start of the test the capillary pressure is a function of the equivalent consolidation pressure and of the state of consolidation of the soil. During the test the capillary pressure decreases, but how large this decrease is we do not exactly know. The decrease of p_k is mainly a function of the compressive force and the state of consolidation of the soil, but to some extent it probably also depends on the above-mentioned angle β . Theoretical considerations led to the belief that for the same value of β and the same state of consolidation of the test specimen (same values of $n_c = \frac{p_e}{p}$), the ratio $\frac{q}{p_e}$ is a constant for each type of soil. This theory was corroborated by the test results shown in Figure E-20; at least it is correct until fairly high values of $\frac{q}{p_e}$ are reached. In case of the tests shown in Figure E-20 the direction of the compressive force and the planes of orientation of the flaky particles are parallel. In 1929 Terzaghi and Janiczek (page 560 in (19)) made similar tests and obtained similar results for the case where the direction of the compressive force is perpendicular to the planes of orientation of the flaky particles. It must be remembered that the curves shown in Figure E-20 apply only to samples in a state of natural consolidation; in case of samples in various states of overconsolidation we get quite different values. The ratio $\frac{q}{p_e}$ was used to compare the strengths of the various types of test specimens, as it was impossible to prepare all these specimens with exactly the same void ratios.

In Figure E-21 are shown some photographs of the test specimens after failure and after air-drying. Samples of type I were subject to rather large deformations in comparison with samples of type II and III, and the determination of the angles of inclination, α , of the planes of failure was uncertain in most cases. On the other hand, samples of type II showed in nearly all cases very distinct and straight lines of failure, from which the angles α could be determined with satisfactory accuracy. Specimens of type III and Vienna Clay showed in some cases a tendency to vertical splitting, which, as shown in Figure E-22, can be explained by the direction of the movement with respect to the orientation of the flaky particles.

The results of the tests are shown in Figure E-23. The values given therein for $\frac{q}{p_e}$, α , and the corresponding angle φ - equation (7) are the average values of the three best tests out of four. The values which could not be determined with satisfactory accuracy are shown in parenthesis. For comparison also the values of φ_o (the proposed new condition of failure, equation (15)), φ_r (Krey-Tiedemann condition of failure, equation (12)), and φ_s (Coulomb condition of failure, equation (8)) are given. In case of φ_r and

φ_s for Little Belt Clay the values given are only approximate; definite values do not exist in this case due to the curvature of the shearing resistance curves.

In case of compression test specimens of type II the orientation of the flaky particles is very nearly parallel to the planes of failure; the compressive strength of these samples therefore corresponds to the shearing resistance determined by means of the usual shearing apparatus. As will be seen, the values of the ratio $\frac{q}{p_e}$ for various types of test specimens vary with about $\pm$ 10% from the values obtained for specimens of type II. In case of Vienna Clay the specimens of type I are strongest and those of type III the weakest, while in case of Little Belt Clay the opposite is true. A similar inconsistency of results is noticed in case of the values of the angles of inclination α and the corresponding angles of internal friction φ. This was rather disappointing as it made it impossible to draw any definite conclusions with respect to the influence of the orientation of the flaky particles on the friction and the cohesion. The tests, however, gave the very satisfactory result that the angles φ for specimens of type II were in complete agreement with the angles φ_o as determined by means of shearing tests and the proposed condition of failure. This agreement is much better than could be expected, and it is not impossible that other values of α and φ will be obtained for different ratios of height to diameter of the compression test specimen. It must be pointed out that in order to obtain consistent results from such tests, extreme care must be taken in the preparation of the test specimens; that is, the ends of the specimens must be absolutely plane and perpendicular to the axis of the specimen.

The tests show definitely that for comparison of the results of shearing tests and unconfined compression tests, the samples for the latter should be prepared in such a manner that the direction of the planes of failure with respect to the orientation of the flaky particles will be the same for both types of tests.

GENERAL CONDITIONS OF FAILURE UNDER ASSUMPTION OF ISOTROPY.

The formulas and methods used for determination of the stress distribution in the soil are in most cases based on the assumption that we are dealing with an isotropic material, and the same assumption is generally made when applying the conditions of failure. In the latter case this assumption is justifiable from a practical standpoint if the condition of failure applies to the planes or directions of minimum strength.

It is a common case in general practice that the foundations will be prepared and the complete structure erected before the void ratio of the soil in the underground has adjusted itself to the new stress conditions. This means that at the completion of the structure we may still have a considerable excess pressure in the porewater. We shall now consider the three cases where this excess pressure in the porewater is negative (I), zero (II), or positive (III).

In case I the most dangerous stress condition will not be reached before the negative excess pressure has been fully equalized, the void ratio correspondingly increased and the cohesion decreased. That is, we cannot count on the original value of the cohesion nor on the temporary increase in the normal effective stresses ($\sigma = \sigma_t - u$, u is negative or a tension). Case I must therefore be treated as case II.

In case II the original stress condition, the changes therein and the state of consolidation of the soil are of such a nature that no changes in the void ratio or in the hydrostatic pressure in the porewater are produced; or, alternatively, the rate of load application is so slow or the soil so permeable that, practically speaking, the void ratio has adjusted itself to the new stress conditions at the moment the full external load has been applied. In this case the hydrostatic pressures in the porewater are equal to the static head, and the effective stresses are easily determined.

For a soil of the same type as Vienna Clay and in a state of natural consolidation the shearing resistance curve is approximately a straight line through origo enclosing the angle φ_s with the abscissae axis (equation (8)). According to the proposed condition of failure, equation (15), the effective angle of internal friction is φ_o. By means of these angles and the relation given by equation (7) we can now construct Mohr's stress circles for any stress condition corresponding to failure. The construction is shown in Figure E-24A, where the line O-B represents the shearing resistance curve. As $\varphi_o < \varphi_s$ the Mohr stress circles will intersect the shearing resistance line and have another straight line $O\text{-}B_1$ as envelope. However, as shown by Terzaghi in (18) and (19), this line is not Mohr's curve of failure, as the points of tangency between this line and the stress circles do not represent stress conditions at failure. From Figure E-24A we obtain the following general condition of failure expressed by the major and minor effective principal stresses σ_1 and σ_3 :

$$\frac{\sigma_1 - \sigma_3}{\sigma_1 + \sigma_3} = \frac{\sin \varphi_s}{\cos (\varphi_s - \varphi_o)} \qquad (20)$$

As $\cos (\varphi_s - \varphi_o)$ is close to unity (for Vienna Clay we have $\varphi_s = 25^\circ 40'$, $\varphi_o = 17^\circ 30'$, and $\cos (\varphi_s - \varphi_o) = \cos 8^\circ 10' = 0.99$), we can neglect this member and obtain thereby Rankine's condition of failure, equation (11), for a cohesionless material with the angle of internal friction φ_s. In this case we can therefore treat a cohesive material as if the cohesion is zero and the angle of internal friction equal to the apparent angle of internal friction.

For the same type of soil in a state of overconsolidation we may, excepting low values of p in comparison with p_m, replace the shearing resistance curve with the straight line A-B in Figure E-18B. In case of soils like Little Belt Clay this substitution can be made only for limited stress intervals. The line A-B intersects the ordinate axis in c_a, the apparent cohesion, and encloses the angle φ_r with the abscissae axis. Taking into consideration that the effective angle of internal friction is φ_o, and provided the normal effective stress p does not exceed the pre-consolidation pressure p_m, we obtain the following condition of failure:

$$\sigma_1 - \sigma_3 = \frac{2 \sin \varphi_r}{\cos(\varphi_r - \varphi_o)} \left(\frac{c_a}{\tan \varphi_r} + \tfrac{1}{2} (\sigma_1 + \sigma_3) \right) \qquad (21)$$

Also in this case we can disregard the member $\cos(\varphi_r - \varphi_o)$ and obtain then Mohr's general condition of failure, equation (9), with the cohesion and the angle of internal friction determined according to Coulomb's law or the Krey-Tiedemann method. In cases I and II it is therefore immaterial if Coulomb's, Krey-Tiedemann's, or the proposed condition of failure is used.

<u>In case III</u> the excess pressure in the porewater is positive right after application of the load. This excess pressure is also equalized in the course of time, whereby the effective normal stresses increase, the void ratio decreases, and the cohesion increases, while the shearing stresses remain constant. The most dangerous stress condition obtains therefore right after application of the load. We will here consider the extreme but also common case where cohesive soils are concerned, that the full additional load is applied before the void ratio has changed at all. Therefore, the cohesion $c = \varkappa p_e = \nu e^{-B\varepsilon}$ remains constant, but the effective normal and shearing stresses are changed due to the new load and the corresponding excess pressures in the porewater. The shearing resistance is given by,

$$s = p \tan \varphi_o + c.$$

As c is a constant, we obtain Mohr's general condition of failure,

$$\sigma_1 - \sigma_3 = 2 \sin \varphi_o \left(\frac{c}{\tan \varphi_o} + \tfrac{1}{2}(\sigma_1 + \sigma_3) \right) \qquad (22)$$

In Figure E-24C the effective normal stress and the corresponding shearing resistance before application of the new load are shown as p and s, respectively. A'-B' is the shearing resistance line according to the Coulomb or Krey-Tiedemann theories, while **A**-B is the shearing resistance line for the constant cohesion, c, and the angle of effective internal friction ϕ_o. If the new load and the corresponding excess pressures in the porewater cause the effective normal stress to be decreased to p', we obtain a shearing resistance s' which is larger than the shearing resistance given by Coulomb's or Krey-Tiedemann's conditions of failure. Conversely, if the effective normal stress is increased to p'' the actual shearing resistance s'' is smaller than the shearing resistance obtained by the Coulomb or Krey-Tiedemann conditions of failure. Whether or not it will be safe to use the last-mentioned conditions of failure will, therefore, depend on the changes in the effective normal stress. However, for large changes in the effective normal stress and as ϕ_o is considerably smaller than either ϕ_s or ϕ_r, the Coulomb or Krey-Tiedemann conditions of failure will, in case III, give a rather poor approximation of the actual conditions.

In the discussion above we have only considered the condition of plane stress, i.e., the influence of the major and minor principal stresses σ_1 and σ_3. As shown by Rendulic (14) and Kjellman (11), the intermediate principal stress σ_2 has a decided influence on the total shearing resistance. In the proposed condition of failure the influence of the intermediate principal stress on the cohesion is actually taken into consideration, as the cohesion is given as a function of the void ratio which again is a function of all three effective principal stresses. For $\sigma_1 = \sigma_2 > \sigma_3$ the void ratio is smaller and the cohesion larger than for $\sigma_1 > \sigma_2 = \sigma_3$. The intermediate principal stress will undoubtedly also influence the frictional resistance, but the available experimental data are too meager to determine the amount of this influence with certainty.

EXCESS HYDROSTATIC PRESSURE IN THE POREWATER AND RAPID SHEARING TESTS.

In order to apply conditions of failure expressed in terms of effective stresses to practical problems, we must be able to determine the excess pressures in the porewater not only in the laboratory test specimen but also in the soil of the actual foundation or earth structure, but our knowledge of the physical properties of soils is still so limited and the problem so complicated that, in most cases, we cannot yet determine these excess pressures with satisfactory accuracy.

In the following we shall only consider the initial excess pressures in the porewater, caused when a change in the total soil stresses takes place. When these initial excess pressures are known the subsequent change in pressure with time, due to adjustment of the void ratio, can be determined by means of Terzaghi's theory of consolidation (22). The initial excess pressure in the porewater depends not only on the change in the total normal stress on the plane of failure but is a function of the changes in all the total principal stresses, of the original stress condition, and of the whole stress history of the soil. Moreover, as shown by Casagrande[2] and Jürgenson[10], if excessive internal displacements causing a partial remolding of the soil take place, the excess pressures in the porewater may again be changed without further change in the total stresses.

The only systematic investigation, so far undertaken, of the relation between the changes in the total stresses and the excess pressures in the porewater is the series of experiments made by Rendulic in connection with a general investigation of the relation between the void ratio and the principal effective stresses. An abstract of the results of these experiments was published in the Proceedings of the International Conference on Soil Mechanics and Foundation Engineering at Harvard in 1936; for further details see (15). In these experiments Rendulic used a tri-axial compression apparatus furnished with a special manometer for measuring the pressures in the porewater. This measurement is not easy as any water conducted to or from the place of measurement will create a flow and thereby change the hydrostatic pressures. The principle of the manometer used by Rendulic is shown in Figure E-25. The hydrostatic pressure in the porewater is balanced by air pressure, transmitted through a mercury manometer. The water and mercury meet in a capillary tube and the plane of contact is kept at constant elevation by regulating the pressure of the compressed air. The mercury manometer will then indicate the air pressure and the hydrostatic pressure in the porewater. The experiments were made with the same Vienna Clay which the writer used for the shearing tests. Cylindrical test specimens, drained through a core consisting of a mixture of sand and mica so proportioned that its consolidation characteristics were nearly identical with those of the clay, were first fully consolidated under various uniform pressures ($\sigma_t = \sigma = \sigma_1 = \sigma_2 = \sigma_3$); thereafter the drainage valve was closed and the above-mentioned manometer connected. Two series of experiments were made; in case I the vertical total stress σ_{t1} was gradually increased and the corresponding porewater pressures measured, while the horizontal total stresses σ_{t2} and σ_{t3} remained constant; in case II σ_{t1} was kept constant while σ_{t2} and σ_{t3} were increased. An example of the test results is shown by the curves in Figure E-26, which have been plotted from data given in (15). The ratio of increase in total stress, $\frac{\Delta \sigma_t}{\sigma}$, is plotted as abscissae and the ratio between the hydrostatic pressure u in the porewater and the increase in total stress $\Delta \sigma_t$ as ordinates. The curves show the relation between these ratios for a sample consolidated under the uniform pressure $\sigma = 2.0$ kg/cm^2. For other values of σ curves of similar shape were obtained; in case I these curves are more or less identical, while in case II scattered results or curves lying above or below those shown were obtained.

As will be seen, the ratio $\frac{u}{\Delta \sigma_t}$ is by no means constant; on the contrary, it is subject to very large variations. A satisfactory explanation of these variations cannot yet be given. It is tempting to explain the initial increase in $\frac{u}{\Delta \sigma_t}$ with $\frac{\Delta \sigma_t}{\sigma}$ and in case II the rise of $\frac{u}{\Delta \sigma_t}$ above unity as a result of progressive disturbance of the soil structure (remolding), but this explanation is contradicted by the subsequent sharp decrease in $\frac{u}{\Delta \sigma_t}$ when the stress condition approaches the condition of failure. Further complications arise when an increase in σ_t is followed by a decrease; it could be expected that u would decrease when σ_t is decreased, but Rendulic found in the single experiment of that kind that u continued to increase. Likewise, the writer found that a decrease of shearing stresses, following an increase, would produce excess pressures in the porewater of the same sign as the pressures caused by the original increase in shearing stresses (see page 69 in (9)).

The extent of the experiments by Rendulic is too limited and the results are not sufficiently consistent to permit the establishment of simplified practical rules for the determination of the excess pressures in the porewater. The tests show, however, that commonly used rules, such as the assumption that the excess pressure in the porewater is equal to the increase in the vertical, total normal stress, or to the increase in the major principal total stress, or that the effective normal stress on the plane of failure and thereby the shearing resistance remains constant, in many cases may be far from correct. This does not mean that these rules may not be useful in practical design, but they must be substantiated and their limits of applicability determined by extensive experiments before they can be used with confidence.

To avoid these difficulties the conditions of failure are often determined by means of rapid shearing tests or by means of simple unconfined compression tests. By either of these tests the void ratio and thereby the cohesion remains constant, while the influence of possible excess pressures in the porewater are included directly in the test results. In other words, we obtain a condition of failure expressed in terms of the total stresses in the sample. The question now arises if the results of such tests can be used with safety in practical design.

The results of slow and rapid shearing tests on Vienna Clay and Little Belt Clay are compared in Figure E-27. The full-drawn lines represent the shearing resistance curves obtained by rapid shearing tests, while the dotted lines represent those obtained by means of slow shearing tests.

From observation of volume changes during slow shearing tests - Figures E-15 and E-16 - we know that the shearing stresses in case of clays in a state of natural consolidation or slight overconsolidation will cause temporary, positive excess pressures in the porewater and in case of strongly overconsolidated clays negative excess pressures. In conformity herewith, the rapid shearing tests give lower values of the shearing resistance than slow shearing tests in case of cohesive soils in a state of natural consolidation or slight overconsolidation and higher values in case of strongly overconsolidated soils.

If the difference in results of slow and rapid shearing tests is solely caused by these excess pressures in the porewater, this difference should vanish when the soil is in such a state of overconsolidation that the shearing stresses do not cause any volume change or any excess pressures in the porewater. However, a comparison of Figures E-15, E-16 and E-27 shows that for such degrees of overconsolidation - Δ H = 0 or points of intersection of the curves representing the equivalent pressures before and after the shearing tests - the rapid shearing tests may give from 10 to 20% higher values for the shearing resistance than those obtained by slow shearing tests (page 104 in (9)). This increase in the shearing resistance is due to the toughness or so-called structural viscosity of the clay, and it will be encountered by all rapid shearing tests irrespective of the state of consolidation of the sample.

In practical design we cannot rely on the increase in shearing resistance caused by structural viscosity; the results of all rapid shearing tests must, therefore, be corrected for this influence whether the sample is in a state of natural consolidation or overconsolidation. Neither dare we rely on the increase in shearing resistance caused by a negative excess pressure in the porewater. Firstly, in the shearing boxes we have a stress condition which approaches pure shear, but in the foundation we may have an entirely different stress condition, causing positive excess pressures in the porewater. Secondly, in case the external load on the foundation should cause negative excess pressures in the porewater, these excess pressures will be equalized and the shearing resistance but not the shearing stresses decreased in the course of time.

These facts are a warning against any indiscriminate use of the results of rapid shearing tests. Such tests should at least be supplemented by other tests in order to determine if the shearing stresses cause positive or negative excess pressures in the porewater and even then it is necessary to take extreme care in the interpretation of the test results.

In case of simple unconfined compression tests the sample is subject to atmospheric pressure and the capillary forces - i.e. negative excess pressure in the porewater - which have replaced the stresses acting on the sample in its original position in the foundation. In this case the void ratio remains constant irrespective of the duration of the test and the influence of the structural viscosity can therefore be avoided. Furthermore, in comparison with rapid shearing tests the simple compression test is easier to make and there is less danger of a partial disturbance of the soil structure during the preparation of the test specimen and of progressive failure during the test. However, we do not know the exact value of the capillary forces and the effective normal stress on the plane of failure, neither at the start of the test nor at the moment of failure. In order to utilize the test results the assumption is made that the shearing resistance of the sample and the shearing resistance at the corresponding point in the foundation are equal.

The disadvantages of the rapid shearing tests and the simple unconfined compression tests are to a large extent avoided in the triaxial compression tests described by Dr. Casagrande (4). In this case the vertical and horizontal external pressures are definitely known and controlled, whereby the original stress conditions in the foundation can be closely approximated in the laboratory. By preventing drainage after this stress condition has been reached and the hydrostatic pressures in the porewater equalized and continuing the test with constant void ratio, we obtain conditions of failure, expressed in terms of total stresses, for a stress condition which is similar to the actual stress conditions in the field. However, it should be borne in mind that conditions of failure expressed in terms of total stresses apply only to a rather limited range of stress changes. We know that failure is governed by the effective stresses, and these stresses should therefore, whenever possible, form the basis of our stability computations.

SLOW PLASTIC FLOW BEFORE FAILURE.

The majority of experiments and papers on the shearing resistance of cohesive soils deal with the maximum shearing resistance under given stress conditions, but so far very little consideration has been given to the slow plastic flow before failure and the decrease of the shearing resistance after failure, although these phenomena have been observed and their importance realized (Terzaghi (17) and (18), Gruner-Haefeli (7)). The continuous, slow plastic flow before failure may, in some cases, cause appreciable horizontal and vertical displacements and thereby increase the settlements ordinarily caused by volume changes and affect the time settlement curves. The decrease of the shearing resistance after failure is very important when we are dealing with progressive failure, which we are in many practical cases.

The commonly used shearing apparatus with a straight movement in one direction is not very well suited for investigation of the slow plastic flow before failure and the shearing resistance after failure, as the cross-section of the test specimen changes slowly during the test and very rapidly after failure has occurred. Apparatus in which the test specimen is brought to failure through torsion are to be preferred for such investigations. Torsion apparatus with full circular cross-section of the test specimen have been used years ago both in this country and in Europe. However, the full circular cross-section of the test specimen has the disadvantage that the shearing stresses and strains and thereby also the volume changes decrease to zero at the center. As the piston through which the vertical normal load is transferred is rigid, this results in an unequal distribution of the vertical normal stresses. These difficulties can, to a large extent, be avoided by choosing a circular ring as cross-section for the test specimen.

Such a ring shearing apparatus, designed by the writer, is shown in Figure E-28. The sample is laterally enclosed between stiff rings and above and below are dentated, porous stones, which allow drainage and through which the torsional forces are transmitted to the sample. These forces are exerted through cables attached to the wheel and running over pulleys to a balanced loading scale. The apparatus is therefore of the constant stress type, which also is best suited for the investigation of the slow plastic flow before failure. For investigation of the shearing resistance after failure, it would be better to have a loading arrangement of the constant stress type devised by Dr. Gilboy and now widely used. With the constant-stress loading arrangement it was necessary to decrease the load by trial until the movement stopped; therefore, it was not possible to investigate the relation between the shearing stress and the velocity of the rapid plastic flow after failure. The sample has an outside diameter of 12 cm and an inside diameter of 6 cm, and the loading arrangement is so proportioned that $S = 20\,\tau_a$, where S is the total shearing load in kg and τ_a the average shearing stress in kg/cm^2. The average shearing stress may, in some cases, differ materially from the actual maximum shearing stress for a given load. However, the maximum shearing stresses may be determined by graphical differentiation of the stress-strain curve for the average shearing stresses (page 119 to 124 in (9)).

Investigations of the slow plastic flow before failure require experiments extended over a very long time, during which the expensive shearing apparatus cannot be used for other tests. This prevented extensive experiments, and the writer has only had opportunity to carry out preliminary tests and one partially complete test. Therefore, the results are not conclusive.

This test was carried out with Little Belt Clay in a state of strong overconsolidation ($p_m = 5.0$ kg/cm^2, $p = 1.0$ kg/cm^2). The load increments were about 20% of the maximum shearing resistance and the time interval between load applications ten days. The complete time displacement curves were determined for each load; it is only the last part of these curves which interests us and this part is shown in Figure E-29. The time after load application is plotted as abscissae and the twist as ordinates. As will be seen, after 240 hours the velocity of the plastic flow has reached a fairly constant value, represented by the tangents shown in the upper left-hand corner. By plotting these velocities against the corresponding average shearing stresses we obtain the slightly curved line in the center. This curve intersects the ordinate axis at a value of the shearing stress approximately equal to 30% of the maximum shearing resistance. This indicates that the continuous, slow plastic flow starts at this stress, which Terzaghi has called the bond resistance. The velocity of the slow plastic flow increases more or less linearly with the shearing stress until this stress approaches the shearing resistance, when a much more rapid increase takes place. However, it must be pointed out that 240 hours after load application the velocity of the plastic flow had not quite reached a constant value; the tests should, therefore, be extended over a much longer period. Furthermore, due to the strong overconsolidation the sample was subject to a slight increase in volume and a corresponding decrease in cohesion during the test. This may have contributed to the velocity of the plastic flow. It would be better to use test specimens in such a state of overconsolidation that the shearing stresses do not cause any changes in void ratio or any excess pressures in the porewater during the test.

DECREASE OF THE SHEARING RESISTANCE AFTER FAILURE.

Examples of complete stress-strain curves for shear are shown in Figure E-30. It was difficult to determine the exact shape of the curves representing the conditions after failure, and this part of the curves is therefore shown in dotted lines. However, the ultimate minimum shearing resistance could, in most cases, be determined with satisfactory accuracy. It is probable that the stress-strain curves after failure to some extent are influenced by the velocity of the displacements.

In case of Vienna Clay in a state of natural consolidation a temporary minimum of 72% of the maximum shearing resistance is reached shortly after failure, whereafter the strength slowly increases to 82%. This increase is due to a slight decrease in void ratio after failure has occurred. Such a temporary minimum was not observed by tests on overconsolidated Vienna Clay nor by any tests with Little Belt Clay. The ultimate minimum values of the shearing resistance of Little Belt Clay varied from 36 to 40% of the maximum values; in case of a very slow test a minimum value of 50% of the maximum value was observed. By Vienna Clay the minimum values of the shearing resistance are reached fairly rapidly after failure, but in case of Little Belt Clay surprisingly large displacements are required before the ultimate minimum values are reached. The angular displacements at this stage correspond to a movement of 3 to 8 cm along the outer rim of the sample.

Ring shearing apparatus similar to the one developed by the writer have also been built by Haefeli (8) and Tiedemann (23). The latter made experiments with both undisturbed and remolded clays and found ultimate minimum values which varied from 30 to 60% of the maximum shearing resistance. In a single case the minimum value was only 20% of the maximum shearing resistance. Tiedemann also found that in case of fat clays the ultimate shearing resistance is not reached before a displacement of 8 to 12, in some cases up to 20 cm, has taken place in the zone of failure.

The question now arises if this decrease of the shearing resistance is due only to a partial destruction of the cohesion or if also the frictional resistance is decreased. In Figure E-31 the curves for the minimum shearing resistance after failure are shown in full-drawn lines and those corresponding to the maximum shearing resistance in dotted lines. From these curves we cannot draw definite conclusions, but we may assume that the proposed condition of failure with respect to the maximum shearing resistance also applies to the minimum shearing resistance after failure and plot the points ($\frac{p}{p_e}$, $\frac{s_{min}}{p_e}$) as shown in Figure E-32. Also in this case do these points lie fairly close along straight lines, although the number of experiments is too small to establish this with certainty. The values of the coefficients of ultimate effective friction and cohesion after failure are given in the tabulations, together with the corresponding values for the maximum shearing resistance. In case of Vienna Clay the coefficient of effective internal friction remains constant after failure, although it suffers a temporary decrease, but this decrease is only apparent and due to a temporary excess pressure in the porewater. On the other hand, the coefficient of effective cohesion is subject to a decrease of 55%. In case of Little Belt Clay both the coefficients of effective internal friction and effective cohesion are decreased, respectively with 49 and 79%. The maximum value of the angle of effective internal friction is 10^{o}, after failure it is only 5^{o}.

The decrease of the shearing resistance after failure is to some extent a thixotropic phenomenon. If we decrease the shearing load until the movement stops and then, after a certain period of rest, increase it until failure again occurs, we find that the shearing resistance now is higher than the ultimate minimum, although the void ratio remains constant. After a certain displacement has taken place the shearing resistance again decreases to its ultimate minimum value. This regain in strength depends on the length of the rest period; in case of Vienna Clay in a state of natural consolidation the regain of strength was complete and consummated in the course of a few hours. On the other hand, Little Belt Clay regained its strength very slowly and only partly. Five days after the movement was brought to a stop the shearing resistance had increased from 40% - the ultimate minimum value - to 50% of the maximum shearing resistance. After a rest period of two months the shearing resistance had increased to 65% of the maximum value, but it is a question if this maximum value ever again will be reached. Inspection of the test specimens indicated that a permanent change of structure had taken place in the zone of failure.

CONCLUSION.

Bearing in mind that the deformations and the ultimate failure of a soil are governed by the effective stresses between the mineral particles, the results of this investigation can be summarized briefly as follows:

1) Preliminary tests indicate that a slow plastic flow may start at comparatively low shearing stresses and that the velocity of this flow increases more or less linearly with the shearing stresses.

2) When the nature of the superimposed load approaches pure shear it will call forth excess pressures in the porewater, which may be positive or negative according to the state of consolidation of the soil and which, upon equalization, will cause a decrease, respectively an increase of the void ratio. However, in case of cohesive soils which have been subjected to certain "critical consolidation procedures", the shearing stresses will not cause any changes in the hydrostatic pressure of the porewater.

3) On basis of the experiments a new condition of failure for cohesive soils has been formulated, according to which the cohesion is solely a function of the void ratio or the equivalent consolidation pressure. This conception results in a larger cohesion, considerably smaller angles of internal friction, and a better agreement with the results of both shearing tests and simple unconfined compression tests than given by the Coulomb or the Krey-Tiedemann condition of failure.

4) In case of unconfined compression tests the direction of the principal compressive force with respect to the planes of stratification in the sample will influence the compressive strength and the inclination of the planes of failure. The samples should be prepared in such a manner that the direction of the planes of failure will have the same relation to the planes of stratification as by shearing tests or in the actual foundation.

5) In case of a complete adjustment of the void ratio to new stress conditions it is immaterial if the Coulomb, Krey-Tiedemann, or the proposed condition of failure is used. However, in case of constant void ratio and cohesion and of changes in the effective stresses, the smaller angles of internal friction given by the proposed condition of failure should be used.

6) The results of preliminary tests by Rendulic indicate that the commonly used, simple relations between an increase in the total stresses and the excess pressure in the porewater, caused thereby, may be far from correct except for certain limited stress combinations. A thorough investigation of the rules governing the variations in these excess pressures in the porewater is therefore urgently needed.

7) In order to avoid the difficult determination of the excess pressures in the porewater, the conditions of failure are often determined by means of rapid shearing tests. It has been demonstrated that rapid shearing tests may give very misleading results, due to the influence of the structural viscosity and of negative excess pressures in the porewater, caused by the shearing stresses.

8) By means of a new ring shearing apparatus it has been shown that the shearing resistance of a cohesive soil is subject to a very material decrease during the rapid plastic flow after failure. This decrease is mainly due to a destruction of the cohesion, although the angle of internal friction of some soils is also decreased after failure. However, if the rapid plastic flow is brought to a partial or complete stop by decreasing the shearing load, the soil will regain its lost shearing resistance partly or completely in the course of time.

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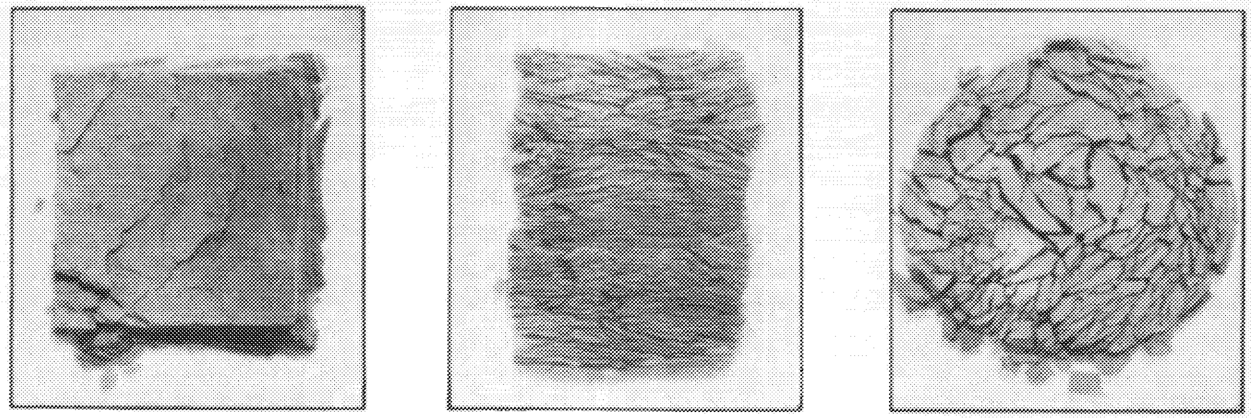


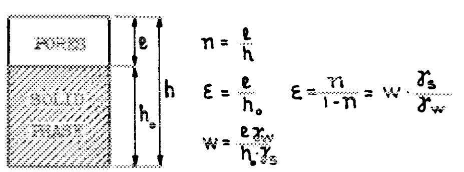
PRESSURE
PERPENDICULAR TO PLANE OF PICTURE.
A

PRESSURE
VERTICAL IN PLANE OF PICTURE.
B

PRESSURE
UNIFORM IN ALL DIRECTIONS.
C

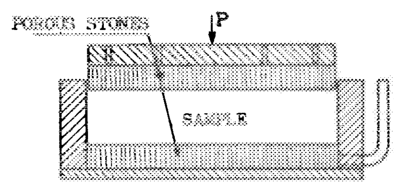
DISINTEGRATION IN WATER OF THIN AIR-DRIED SLICES OF VIENNA CLAY.

FIG. E-1.



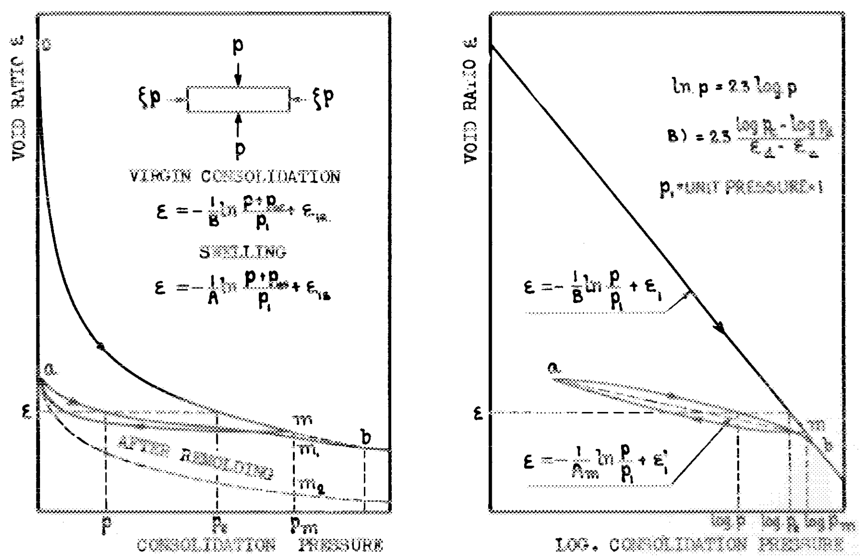
POROSITY - VOID RATIO - MOISTURE CONTENT.

FIG. E-2.



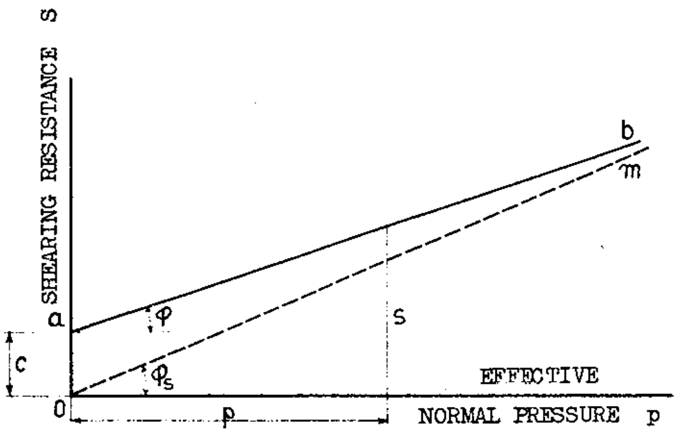
CONSOLIDATION TEST.

FIG. E-3.



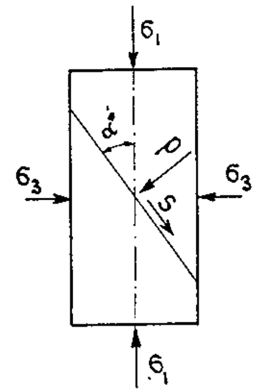
TYPICAL PRESSURE - VOID RATIO - DIAGRAM.

FIG. E-4.



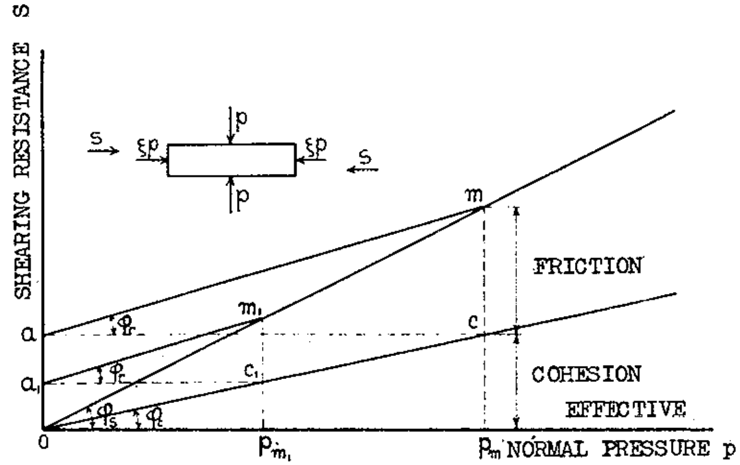
COULOMB'S CONDITION OF FAILURE.

FIG. E-5.



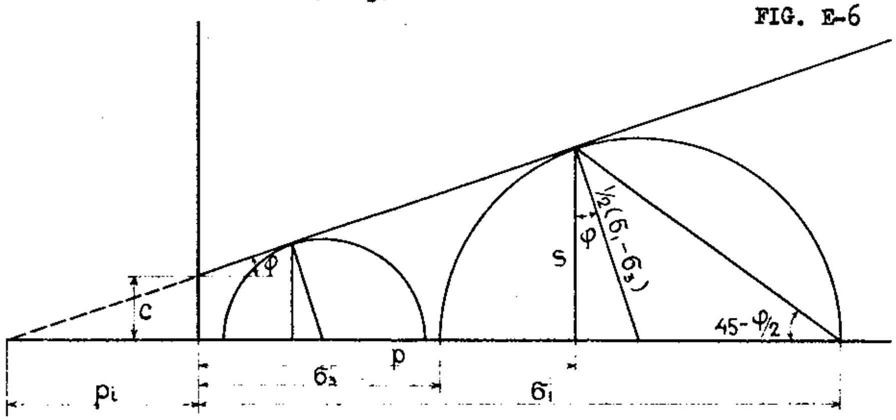
INCLINATION OF PLANES OF FAILURE.

FIG. E-6



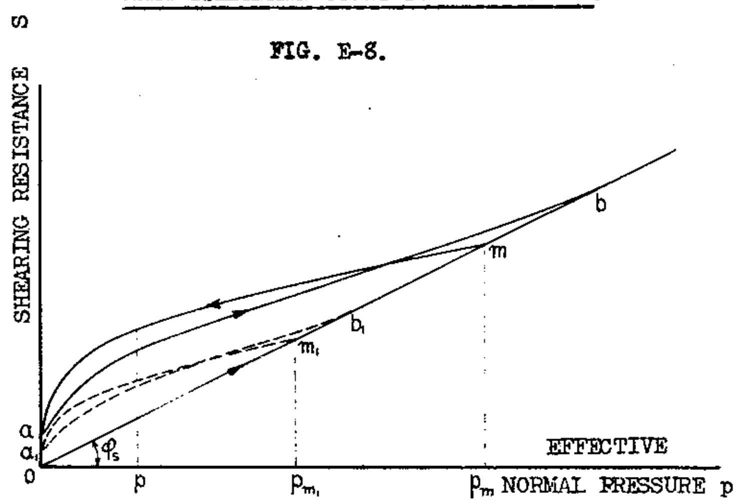
KREY-TIEDEMANN CONDITION OF FAILURE.

FIG. E-8.



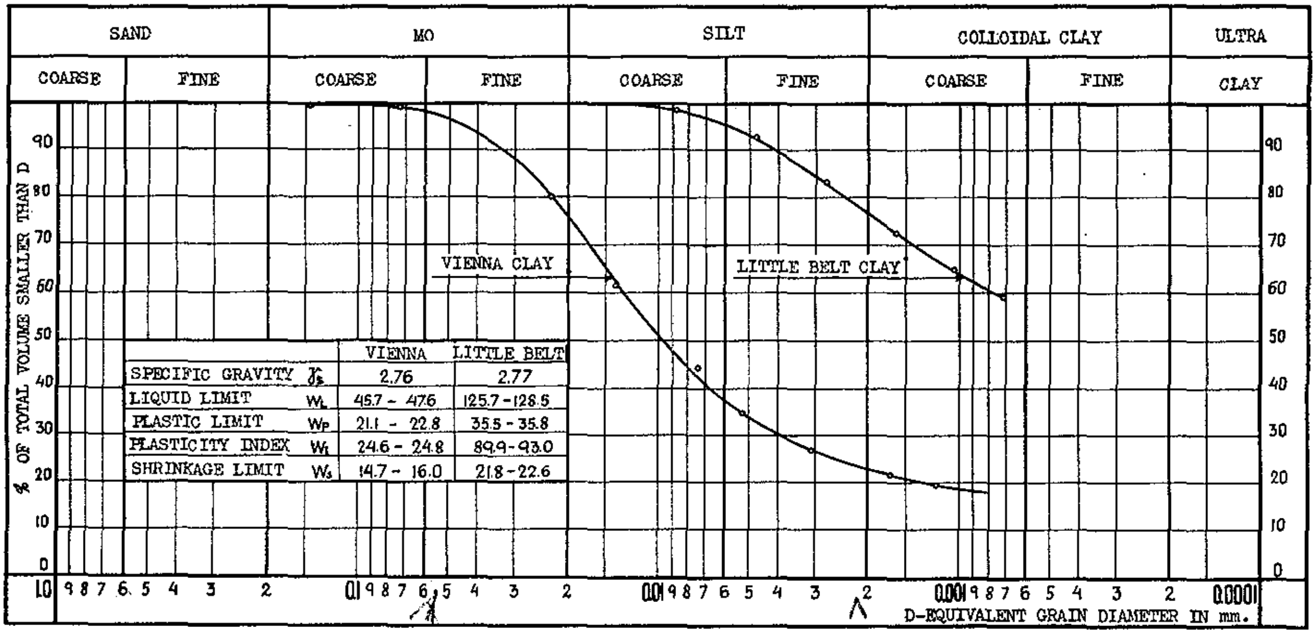
MOHR'S CONDITION OF FAILURE.

FIG. E-7.



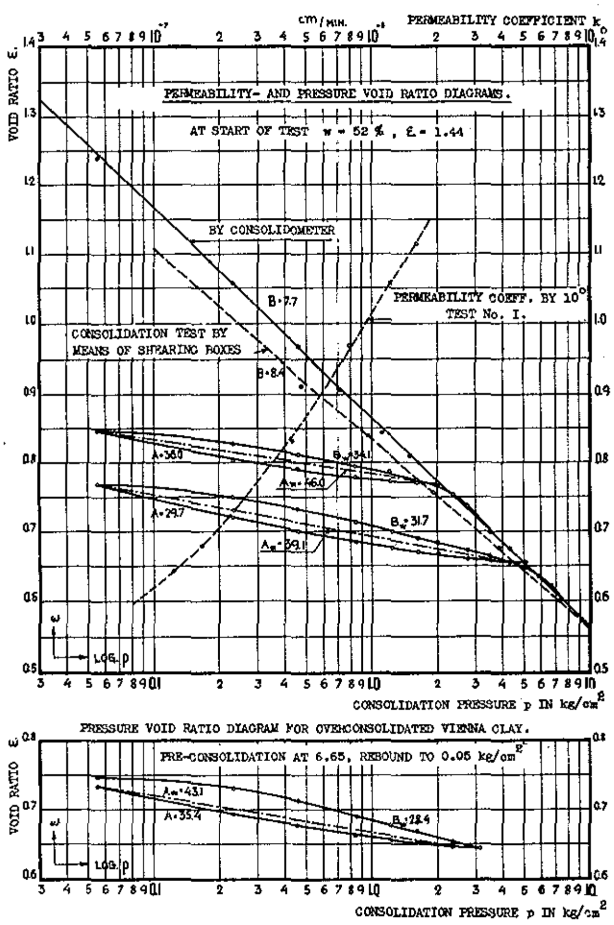
SHEARING RESISTANCE HYSTERESIS LOOP.

FIG. E-9.



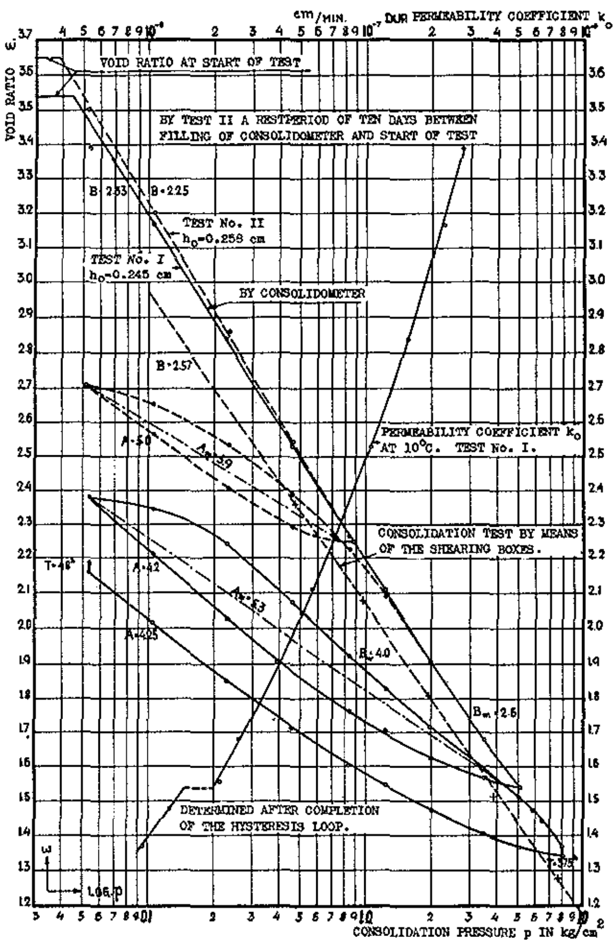
GRAIN-SIZE DISTRIBUTION CURVES.

FIG. E-10.



CONFINED COMPRESSION TESTS ON VIENNA CLAY.

FIG. E-11.

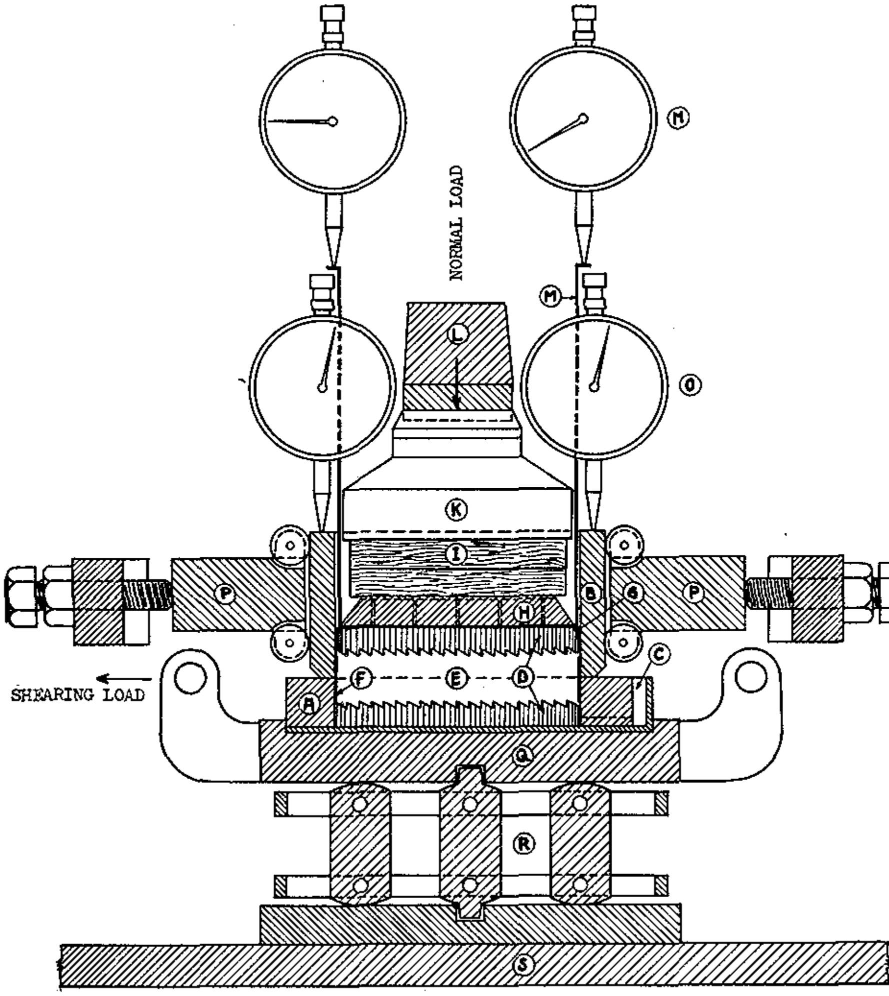


CONFINED COMPRESSION TESTS ON LITTLE BELT CLAY.

FIG. E-12.

LEGEND.

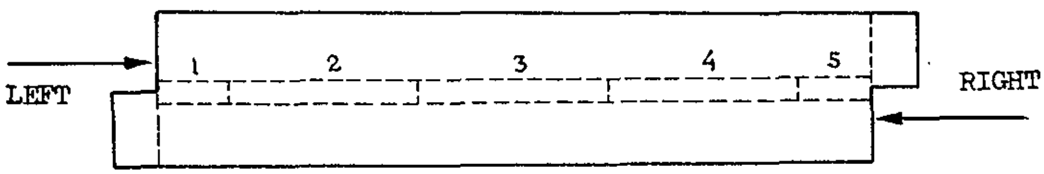
A BASE OF SHEARING BOX
B FRAME OF SHEARING BOX
C WATER OUTLET
D DENTATED POROUS STONES
E SOIL SAMPLE
F GREASED FILTER PAPER
G COTTON
H PERFORATED PLATE
I WOOD INLAYS
K PISTON WITH STEEL KNIFE EDGE
L LOADING BEAM
M INLAY WITH EXTENSOMETER STOPS
N EXTENSOMETER - SAMPLE
O EXTENSOMETER - FRAME
P GUIDES WITH BALL BEARINGS
Q SLEDGE
R ROCKER
S TESTING BENCH



KREY SHEARING APPARATUS WITH TERZAGHI SHEARING BOXES.

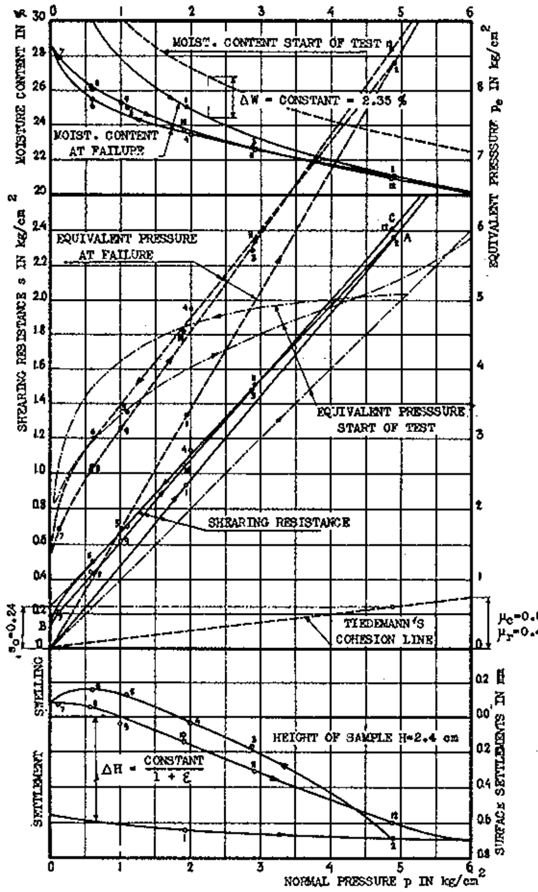
FIG. E-13.

A STRIP 3 mm THICK AND 2 cm WIDE IS CUT FROM THE ZONE OF FAILURE AND DIVIDED INTO FIVE SECTIONS. THE MOISTURE CONTENT OF SECTIONS ONE AND FIVE IS CORRECTED FOR WATER ABSORBED FROM THE FILTER PAPER.



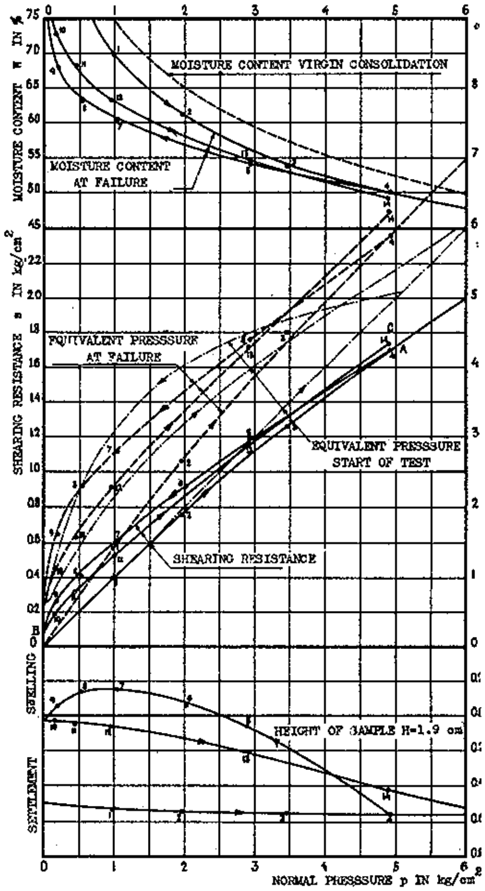
DETERMINATION OF AVERAGE MOISTURE CONTENT OF SHEARING TEST SAMPLES.

FIG. E-14.



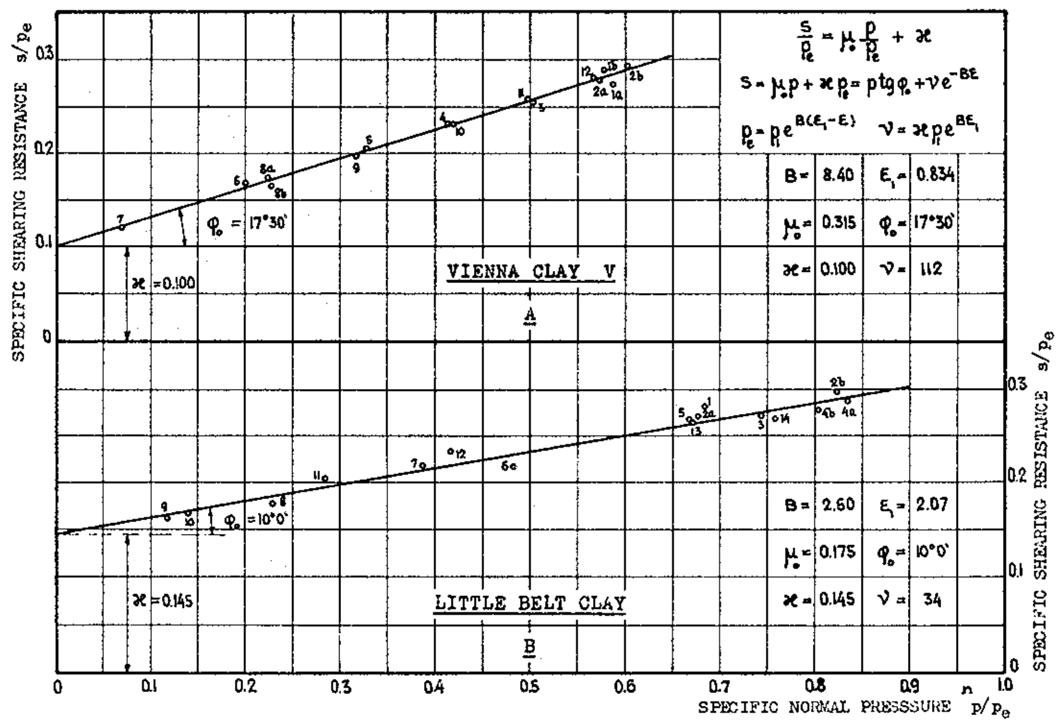
SLOW SHEARING TESTS WITH VIENNA CLAY V.

FIG. E-15.



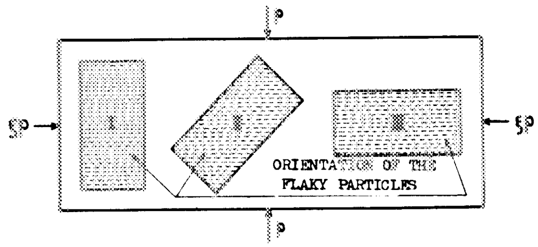
SLOW SHEARING TESTS WITH LITTLE BELT CLAY.

FIG. E-16.



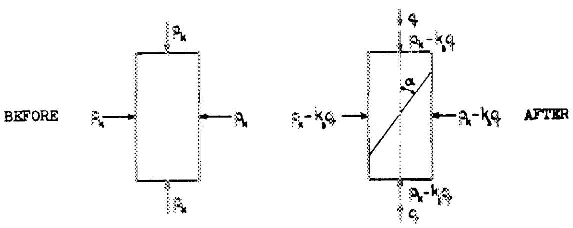
GRAPHICAL DETERMINATION OF THE SHEARING RESISTANCE COEFFICIENTS.

FIG. E-17.



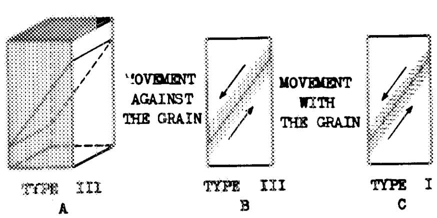
PREPARATION OF COMPRESSION TEST SPECIMENS.

FIG. E-18.



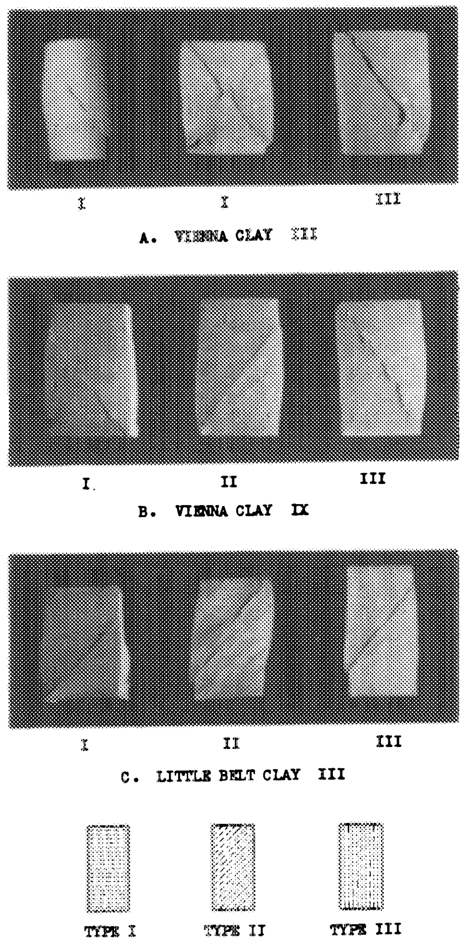
CHANGES IN CAPILLARY PRESSURE.

FIG. E-19.



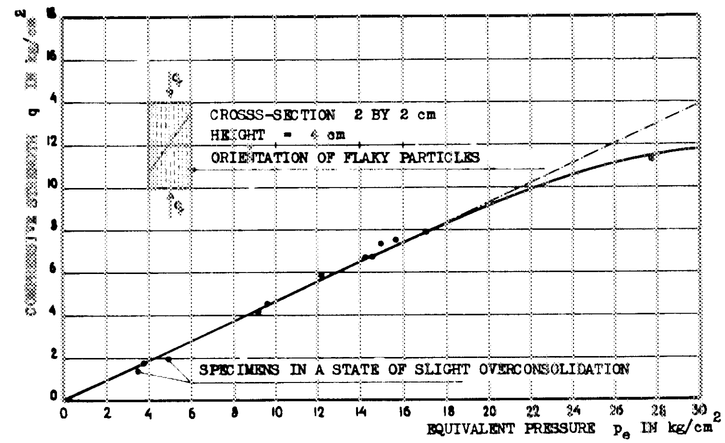
INFLUENCE OF STRATIFICATION.

FIG. E-22.



COMPRESSION TEST SPECIMENS AFTER FAILURE.

FIG. E-21.



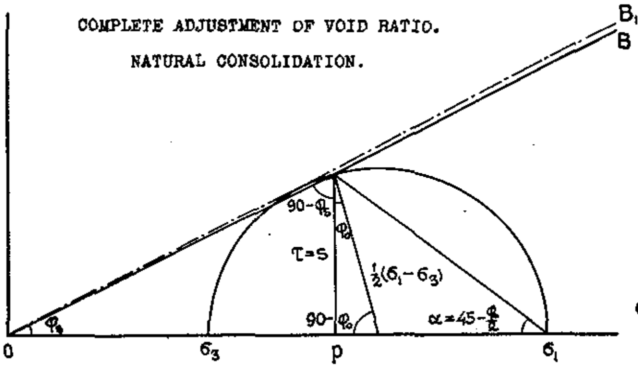
RELATION BETWEEN COMPRESSIVE STRENGTH AND
EQUIVALENT CONSOLIDATION PRESSURE.

FIG. E-20.

	VIENNA CLAY III			VIENNA CLAY IX			LITTLE BELT CLAY		
	I	II	III	I	II	III	I	II	III
q/p_e	0.54	0.50	0.46	0.48	0.44	0.42	0.38	0.41	0.46
RELATIVE STRENGTH	108	100	93	109	100	96	93	100	112
α	36°	36°	35°30'	(39°)	35°30'	36°	(43°)	40°15'	(42°)
$\varphi = 90-2\alpha$	18°	18°	19°	(12°)	17°	18°	(4°)	9°30'	(6°)
φ_e	VIENNA CLAY I	18°		VIENNA CLAY V	17°30'		LITTLE BELT CLAY II	10°	
φ_r		25°			24°			(17°30')	
φ_s		26°34'			25°40'			(20°)	

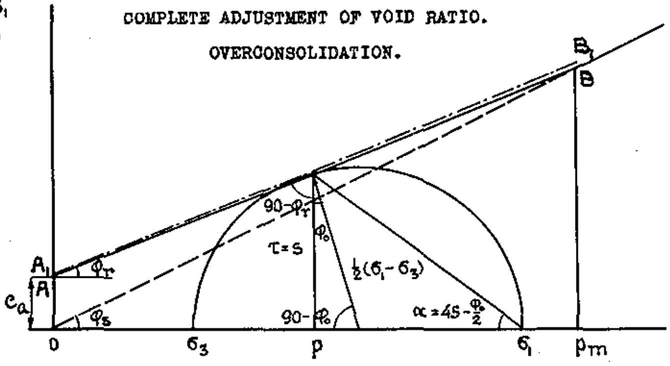
SUMMARY OF RESULTS OF COMPRESSION TESTS.

FIG. E-23.



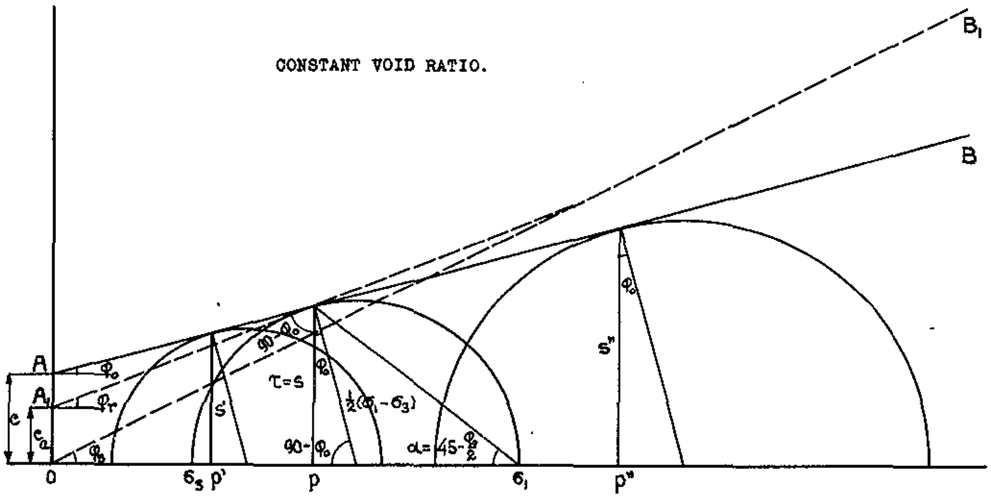
CONDITION OF FAILURE : $\frac{\sigma_1-\sigma_3}{\sigma_1+\sigma_3} = \frac{\sin\varphi_s}{\cos(\varphi_s-\varphi_o)}$

FIG. E-24A.



CONDITION OF FAILURE : $\sigma_1-\sigma_3 = \frac{2\sin\varphi_r}{\cos(\varphi_r-\varphi_o)}\left[\frac{c_a}{\tan\varphi_r} + \frac{1}{2}(\sigma_1+\sigma_3)\right]$

FIG. E-24B.

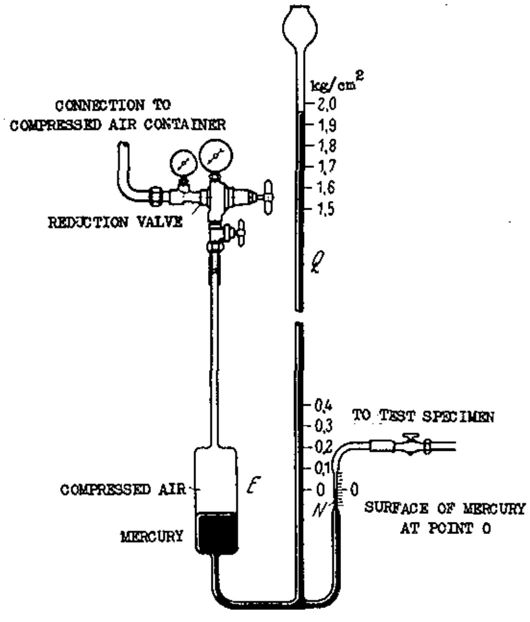


CONDITION OF FAILURE : $\sigma_1-\sigma_3 = 2\sin\varphi_o\left[\frac{c}{\tan\varphi_o} + \frac{1}{2}(\sigma_1+\sigma_3)\right]$

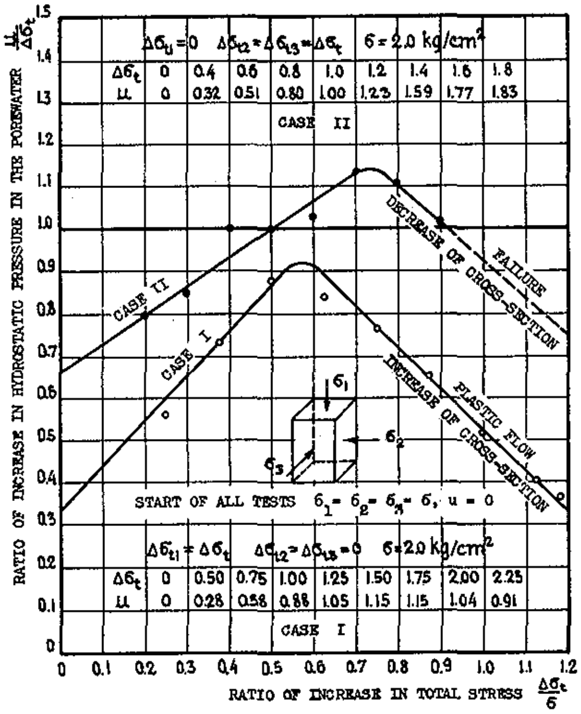
FIG. E-24C.

<u>GENERAL CONDITIONS OF FAILURE UNDER ASSUMPTION OF ISOTROPY.</u>

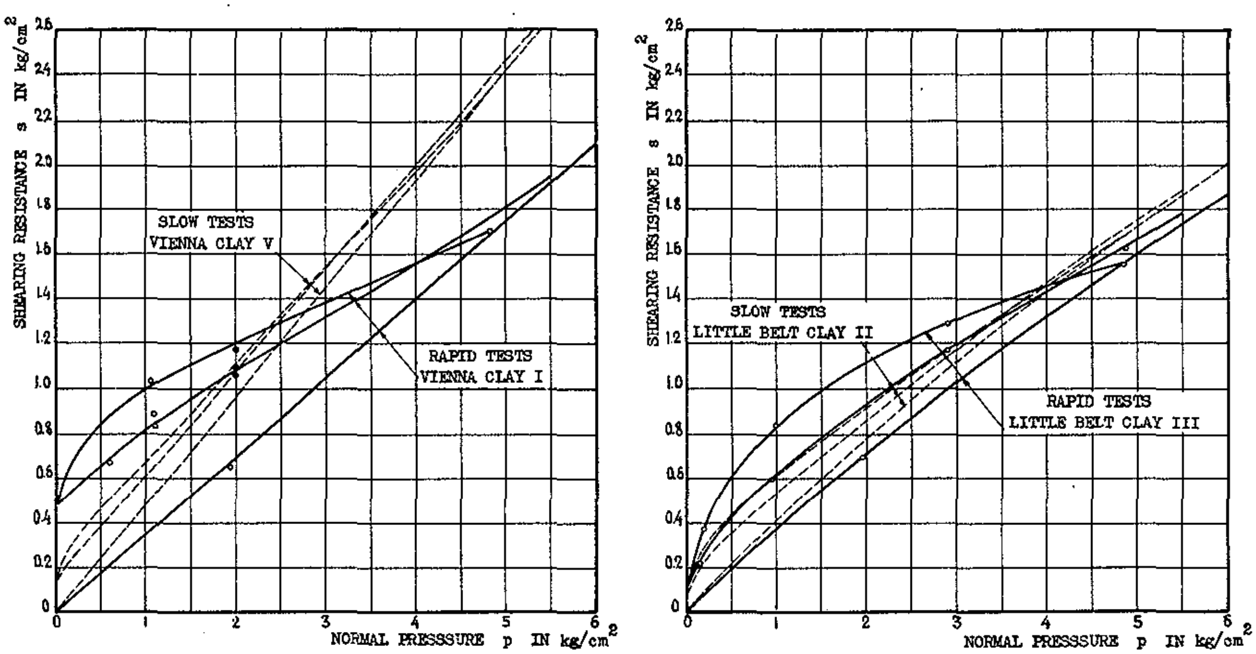
FIG. E-24.



MANOMETER BY RENDULIC FOR MEASURING
HYDROSTATIC PRESSURES IN THE POREWATER.
FIG. E-25.

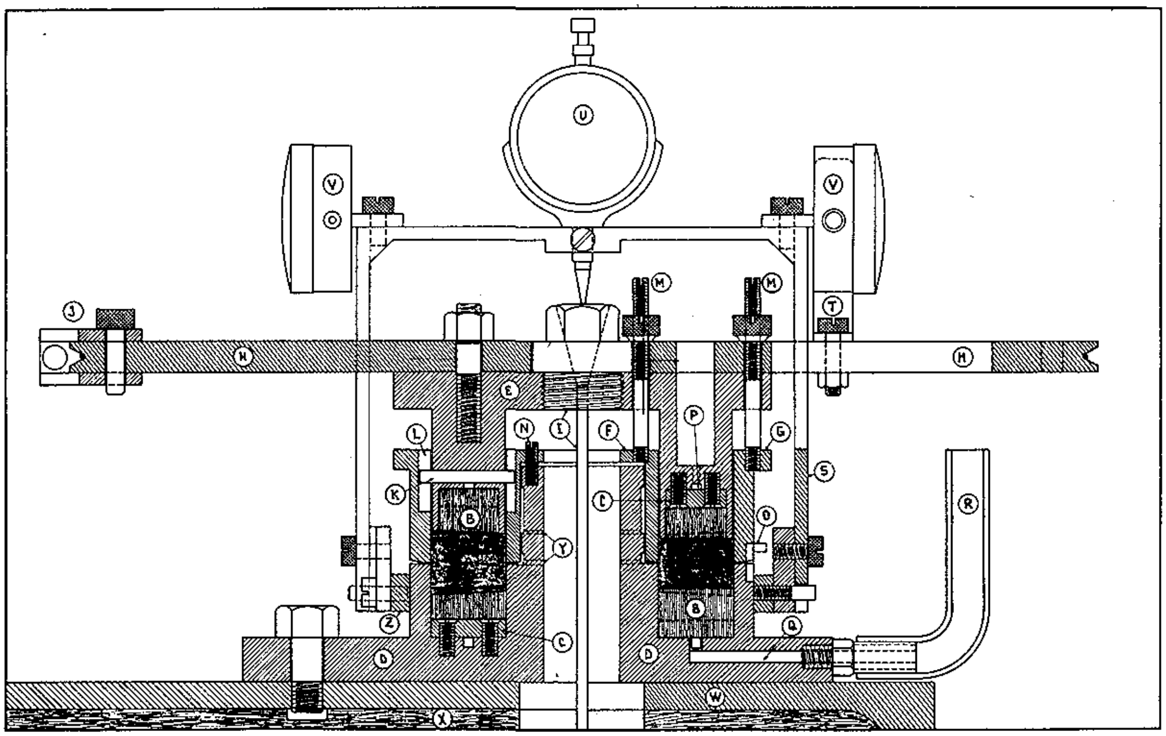


RELATION BETWEEN EXCESS HYDROSTATIC PRESSURES IN
THE POREWATER AND INCREASE IN TOTAL STRESSES
ACCORDING TO EXPERIMENTS BY RENDULIC.
FIG. E-26.



A - VIENNA CLAY.　　B - LITTLE BELT CLAY.

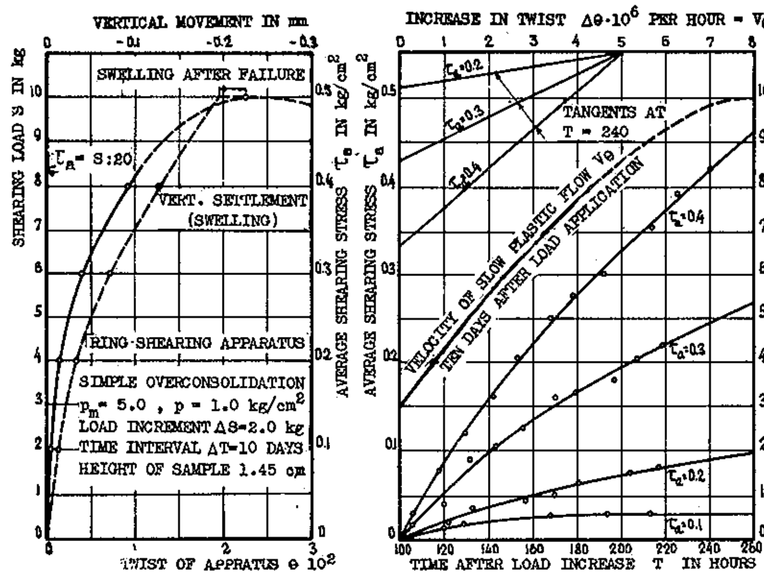
COMPARISON BETWEEN RESULTS OF SLOW AND RAPID SHEARING TESTS.
FIG. E-27.



A	TEST SPECIMEN	J	CONNECTION FOR WIRE ROPE	S	CARRIER FOR STRAIN GUAGES
B	DENTATED POROUS STONES	K	GUIDE PINS	T	STOP FOR STRAIN GUAGE
C	RIBS	L	GUIDE GROOVES	U	VERT. STRAIN GUAGE
D	BASE	M	VERT. ELEVATION SCREWS	V	HORIZ. STRAIN GUAGE
E	RING PISTON	N	FIXATION SCREWS	W	BASE PLATE
F	INNER RING	O	FIXATION PINS	X	TESTING BENCH
G	OUTER RING	P	UPPER WATER OUTLET	Y	WATER OUTLETS
H	LOAD TRANSMISSION WHEEL	Q	LOWER WATER OUTLET	Z	FIXATION RING
I	LOADING SCREW AND ROPE	R	GLASS TUBE		

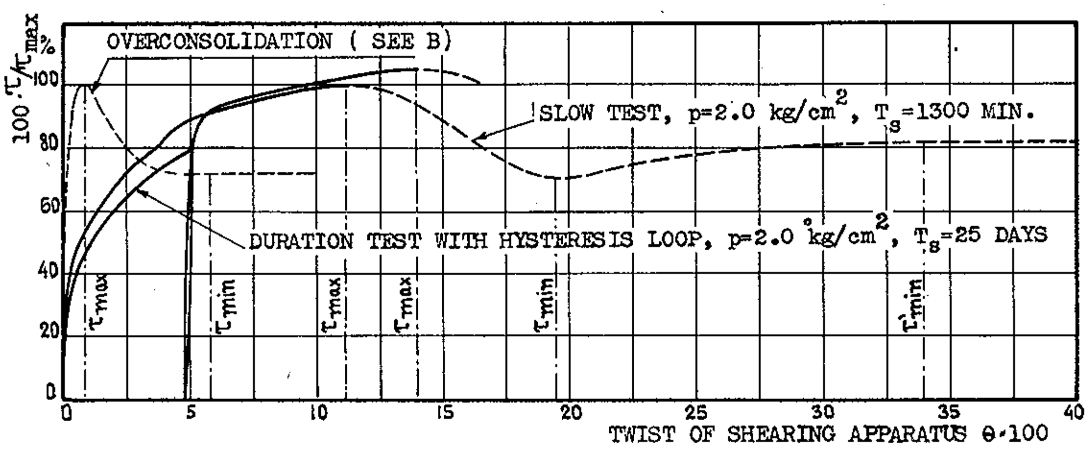
RING SHEARING APPARATUS.

FIG. E-28.



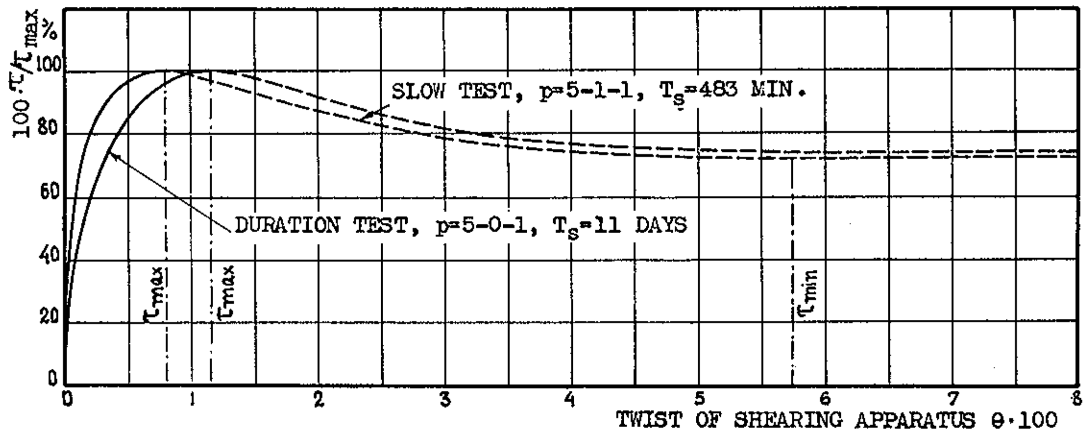
LITTLE BELT CLAY - VELOCITY OF SLOW PLASTIC FLOW BEFORE FAILURE.

FIG. E-29.



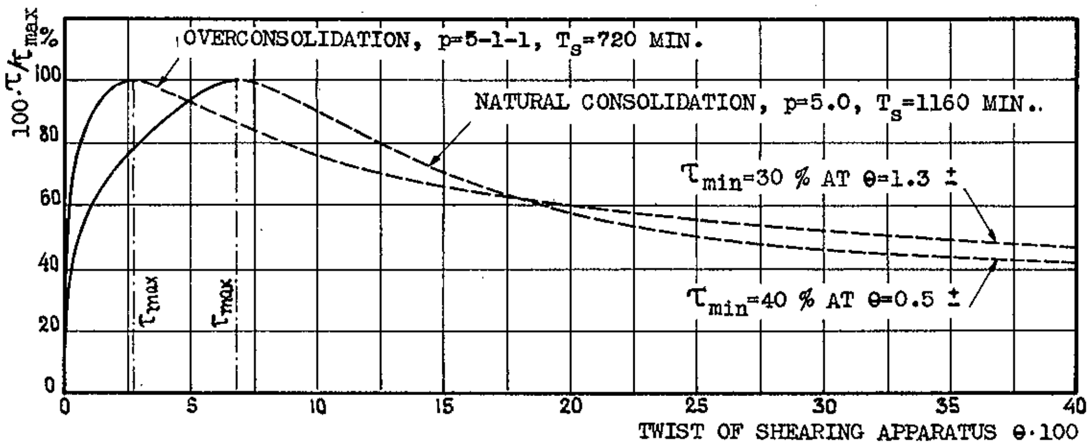
VIENNA CLAY - NATURAL CONSOLIDATION.

A



VIENNA CLAY - OVERCONSOLIDATION.

B

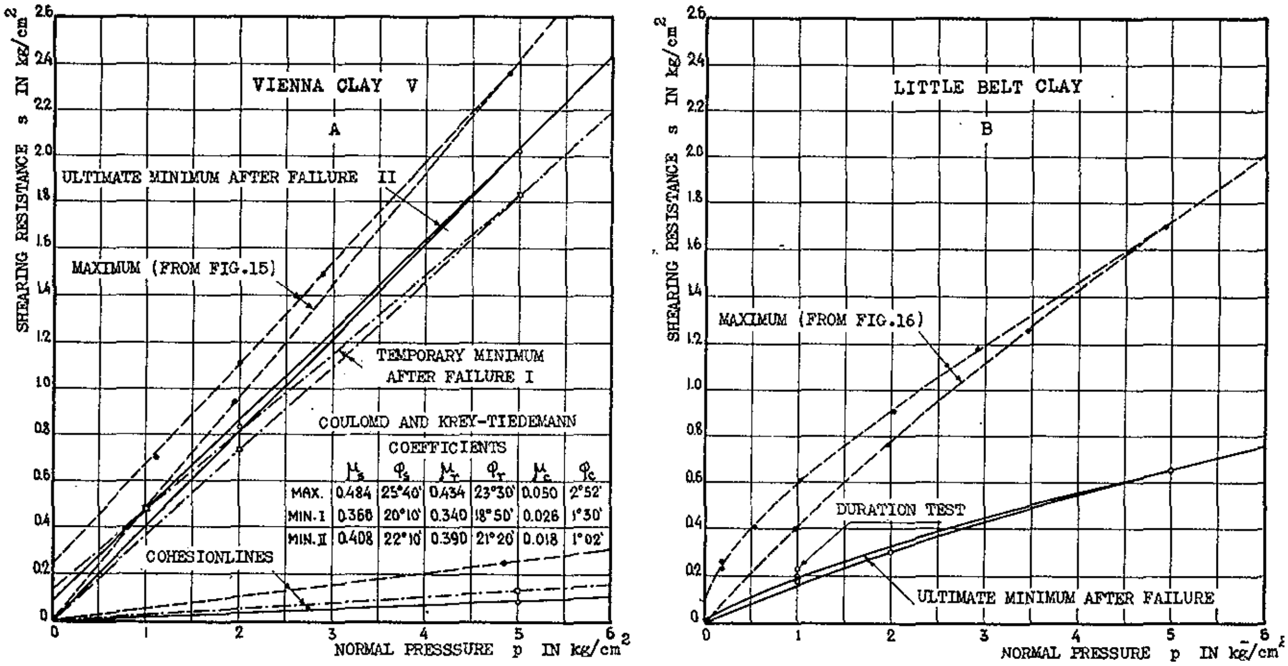


LITTLE BELT CLAY.

C

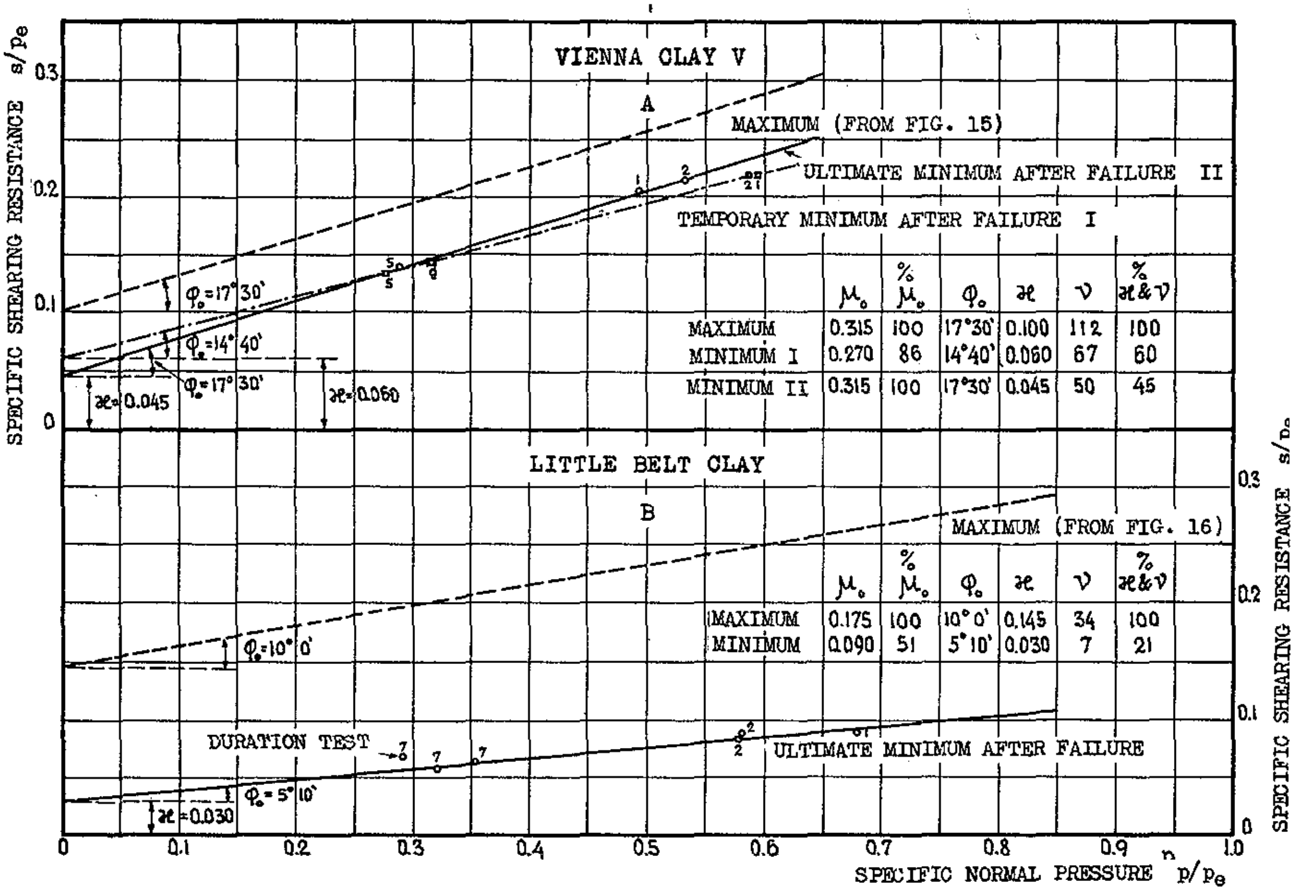
STRESS-STRAIN CURVES FOR SHEAR BEFORE AND AFTER FAILURE.

FIG. E-30.



SHEARING RESISTANCE CURVES BEFORE AND AFTER FAILURE.

FIG. E-31.



DECREASE OF COHESION AND FRICTION AFTER FAILURE.

FIG. E-32.

GENERAL REMARKS ON SOIL MECHANICS RESEARCH

by

Dr. Glennon Gilboy

Professor of Soil Mechanics
Massachusetts Institute of Technology, Cambridge, Mass.

GENERAL REMARKS ON SOIL MECHANICS RESEARCH

Dr. Glennon Gilboy, Professor of Soil Mechanics, Massachusetts Institute of Technology

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Inasmuch as the other papers presented at these meetings deal with important technical problems, I believe it will help to balance the program if I keep away from technicalities as much as possible and confine my remarks to certain general aspects of the work we are proposing to undertake. I should like first to discuss the attitude we should try to adopt, and then to take up certain of the problems we are proposing to attack and analyze some of the difficulties we must be prepared to meet.

In the matter of attitude, there is at one extreme the idea that elaborate soil studies can be made the basis for exact analysis; that eventually it will be just a question of determining a lot of coefficients, making a lot of computations, and coming out with the right answer. I do not believe that in the enlightened group here assembled there are many representatives of this school of thought; but I know from experience that it exists. About eight years ago I heard a man quite prominent in soils work explaining some charts to a group of highway engineers. One of the latter described a cracking phenomenon he had observed on a concrete road. The soils man asked a few questions, then concluded with great positiveness: "It's your concrete, man, it's your concrete! If you fellows would develop the exact knowledge of concrete that we have of soils, you wouldn't get into so much trouble!" I only wish I could agree with him.

At the other extreme is the school of thought, considerably larger numerically, which holds that Soil Mechanics is just tommyrot; that the building of dams is a job for hard-bitten he-men, not for dreamy-eyed scientists watching little dials go around in a laboratory. A very recent example comes to my mind. Last fall I was asked to present a short paper at the meeting of the Soil Mechanics and Foundations Division of the American Society of Civil Engineers reviewing some of the main achievements of Soil Mechanics. I chose as my underlying theme the application of scientific methods of investigation and tried to show how those methods improved our understanding of a number of different problems. For an example, I need go no farther than my own personal experience. As a graduate student, I had a good grounding in such subjects as Mechanics, Hydraulics, Structures, and so on, but my knowledge of the various problems connected with soil behavior was very hazy and inaccurate.

It would probably have remained so for some time had I not had the opportunity, through study with Dr. Terzaghi, through association with Dr. Casagrande and other colleagues, and through my own investigations, to develop some intelligent understanding of the subject. Consequently, there is no question in my mind as to the benefits to be derived from scientific study of soil problems. I know I learned a great deal, and I am sure that every one here today who has had similar special training will agree that he has benefited by it in a similar manner.

A couple of months afterward I found my paper reprinted, with some omissions, in an English journal, and in the same issue I found an editorial which dragged me unmercifully over the coals. The gist of it was that the scientific investigations I mentioned had not really added anything to existing knowledge; that American engineers were a pretty ignorant crew, and might be taken in by such truck, but that no clear-thinking Britisher, with centuries of fine engineering tradition behind him, could possibly be deluded into thinking that the methods of the past could be improved.

I do not for a moment believe that this editorial reflected the opinion of progressive British engineers, but I do think it was significant in indicating a rather prevalent attitude of mind: "We have done it this way for so and so many years, therefore it must be right and nobody can tell us anything new."

Those, then, are the two extremes, both of which are to be avoided. Any one who thinks that our present program of research, or any subsequent program, will result in reducing dam design to routine practice -- a book of rules entitled "How to Design Dams in Fifteen Easy Lessons" -- is doomed to disappointment. Conversely, any one who thinks he knows so much about dam design that the proposed program of research, or any such program, cannot possibly add to his knowledge, is deceiving himself and deceiving the public.

The proper attitude, in my opinion, is to expect that, as a result of these and similar studies, we shall be able to decrease in some degree the range of uncertainty in design and, most important of all, that every one concerned will derive considerable personal benefit, in the form of improved judgment and understanding.

The scope of the program for discussion at this conference is, I think you will agree, rather breath-taking. We are planning to touch on almost all aspects of embankment design and to consider methods of attack on some problems of a highly complicated nature. While this may seem overly ambitious, I think we will find it helpful to obtain a clear picture of the whole field as a background and then to do as much as we can to fill in details.

In my original memorandum I stressed the necessity for field observations. I consider this work of great importance and I think that in this organization we have an unusually good opportunity for performing it. Our theoretical studies and laboratory tests may enable us to estimate how our structures <u>should</u> act, but we have not proved our point until we can show, by actual observation, that they really <u>do</u> act as we expect. The mere fact that an embankment stands up is not a sufficient criterion. It may be standing for quite a different reason from what we assume; it may be standing purely by the grace of God. Until we have verified our predictions reasonably well, we have only half done the job.

Among all the various possibilities for prediction and measurement, I should say that we have progressed farthest in the case of settlement due to direct compression of a foundation layer. When underground conditions are not too complicated, consolidation tests and settlement analyses should give a fair indication of what is liable to happen. The field observations require nothing more than a good set of reference points. Studies of this kind were made by Mr. Philippe on several of the Muskingum Dams, with excellent results. It goes without saying that such studies should always be carried out when compressible foundation layers are involved. Even when conditions are complicated, some estimate of settlement should be attempted and field observations should be made with especial care in order to ascertain the range of error.

When foundation movement involves lateral displacement, we are up against an entirely different situation. It is a stress-strain problem, in which we don't know a great deal about either the stresses or the strains. Jürgenson's computations have been very helpful, but the trouble is that in order to get anywhere at all, he had to assume a definite loading system, and also had to assume the foundation to be elastic. The elastic assumption is not as far off as might at first be thought, especially for conditions which are not too close to ultimate failure. The loading system, on the other hand, is not at all easy to pin down. The stresses at the base of an embankment are inclined in direction, and their magnitude and distribution are affected not only by the size of the structure and the character of the material, but also by the method of construction. Furthermore, the stress distribution will be materially altered by deformations of the foundation as it adjusts itself to the load.

Various mathematical approaches have been tried in order to get some idea of the nature of the stresses, but it must be admitted at once that existing methods are not satisfactory. In view of the fact that the mass design of the whole embankment is often governed by the strength of the foundation, the need for better information is obvious. Obtaining such information is not an easy task. Measurements of deformation of existing embankments are of considerable value, especially when the movements are relatively large. On the other hand, such measurements do not tell the whole story, because they give only the end result. We know that the measured movement is the result of strains in the foundation material, caused by stresses induced by the superimposed loads, but we cannot be sure, so to speak, where stress leaves off and strain begins. In other words, there are an infinite number of stress-strain combinations which might have produced the observed movement and it is difficult to decide which particular combination actually exists in a given case.

Oneway of separating these effects would be to measure the stresses independently. Theoretically, this is a very fine approach. Practically, it is chock full of difficulties. We have on our agenda for discussion the development of pressure cells. I do not wish to sound discouraging, but I might mention that one of the first items I studied when I began my acquaintance with Soil Mechanics was this very problem. Dr. Terzaghi and I argued and fretted and stewed over the thing far into the night on many occasions. Our progress may be judged by the fact that four years later, when the retaining wall laboratory was built, we installed Goldbeck cells. And I haven't seen many signs of progress in the nine years which have passed since then.

Let us consider some of the qualifications a pressure cell must meet. First of all, it must be simple and rugged, not only the cell itself, but all its appurtenances. Otherwise it will soon go out of commission in a field installation. One drawback of the Goldbeck cell, for example, is the air pipe leading to it, which is not only easily damaged during construction, but also may be ruined by settlements of the fill after completion. In the Miami Conservancy Dams, in spite of elaborate precautions, the only cells which remained in service for any length of time were in a group installed in the solid concrete of an abutment wall.

Second, the cell must be reliable -- that is, it must hold its calibration over a long period of time, under different conditions of pressure and temperature. In the M. I. T. installation we found that some of the cells would now and then go completely haywire. Sometimes they registered no pressure at all when we knew they were under pressure; sometimes the signal lamp would fade out gradually, instead of going out suddenly. We were never sure what they were going to do, so we soon ceased to rely on them.

Third, the operation of the cell must be such that there is no appreciable external movement required to register the pressure; otherwise the movement will change the pressure which the cell is intended to measure.

Now let us suppose that we have a cell, something like the Goldbeck, perhaps, but perfected to comply with the above conditions. We still have not solved the problem. Our pressure cell measures pressure, but it does not measure shear. If we place three cells at right angles we can get the pressures on those planes, but we have not determined the state of stress in the soil at that point unless we happened to have hit on the principal planes. So what we really need is a cell which measures shear as well as pressure, or else some kind of spherical arrangement which would allow us to measure the pressure variation all over the surface.

In addition to all these things, there is the fundamental question as to the effect of the presence of such a device within a soil mass: Is the pressure it measures the same as would have existed if it were not there? Unless we can be sure on this point, the most accurate type of measuring device would be worthless.

So much for pressure measurement. I think you will agree that it gives one something to think about. The remaining aspect of the displacement problem is, of course, analysis of the deformations produced by the given system of stress, which in turn requires a knowledge of the stress-strain characteristics of the material. This is another item on our agenda, and, I can assure you, is in the same class as stress measurements as far as complexities are concerned. I will say, however, that some progress has already been made along this line, and we can see our way clear to obtaining additional information. In other words, I am much more hopeful about this than I am about the pressure cell.

The stability of the slopes of the embankment itself is an item to which considerable thought has been given in recent years. The studies of slides made by the Swedish Geotechnical Commission form a useful background, but it must be remembered that dam slopes are by no means identical with those studied by the Commission, so that routine application of the circular-arc analysis is not justifiable. The findings of the Commission were based on exhaustive investigation of slides which had actually occurred. No such body of correlated information has been obtained on dams. If we wish to follow the lead of the Swedish engineers, we should follow it all the way; first find out how the failures take place, and then theorize about them afterward. I have seen some statical analyses which I am sure bore no relation to reality; a circle starting in the downstream shoulder of sand and gravel, cutting through a compact, impervious, cohesive core, then into a foundation layer of stiff clay, then up again through another sand and gravel shoulder, with detailed computations involving weights, friction angles, cohesion values, and what not, ending up in a factor of safety. It all looks very professional, but I am afraid it does not mean much.

Last fall, in the Binghamton District, we attempted to develop an analysis in which the action of the core and the shoulders were considered separately. There seems to be some justification for this approach, and we got some interesting preliminary results. However, we found that so many assumptions had to be made, and it was so difficult to decide which assumptions might be correct, that the validity of the results would finally end up to be merely a matter of opinion. I feel that there is little use trying to push the refinements of statical analysis any farther at present. What we need now is experimental evidence, in order to put the whole matter on a sound basis.

I believe that a great deal can be done along these lines with test embankments of moderate height, built inside a diversion cofferdam so as to enclose a small pool, as suggested in my earlier memorandum. Not all sites are adapted to this scheme, but some I have seen are ideally suited. At Wappapello, for instance, the river valley is quite deeply indented below the flood plain, and not very wide, and the banks are relatively impervious. Construction of a test dam would not be difficult nor expensive, and a great deal of information could undoubtedly be derived from it.

The reason why the field, rather than the laboratory, seems indicated for stability studies is that almost any cohesive material can be excavated to vertical slopes to a depth of 6 ft. or more. This height, which is already about the limit for laboratory work, must be exceeded considerably before sliding forces become effective. On the other hand, laboratory models will probably be very well adapted to studies of drawdown effects and toe erosion, both of which are important elements of the stability problem. In fact, drawdown conditions are often the governing factor in the design of the upstream slope, and we should know considerably more about them than we do. In particular, we should be able to estimate the extent to which the saturation line in the dam lags behind a falling reservoir level. I worked out an approximate theoretical solution, in connection with the Binghamton investigations, but as yet there has been no opportunity to check it experimentally, and I am putting no faith in it until it has been verified.

As to the problems of seepage, I think we may say that the basic theory and the technique of permeability tests are both fairly well developed. Our weakest point is in estimating the direction and amount of seepage in irregularly stratified and lenticular deposits in the foundation. I do not believe that more elaborate programs of sampling are the right answer, nor that field models will give us what we want. I think the solution lies along the line of developing reliable and inexpensive methods of testing the permeability of the soil in place, in large masses, such as by pumping from wells or pits. Also, it is important to verify predicted seepage quantities by observations on completed structures. In some dams this is a rather difficult task, in others it is quite simple. At Great Salt Plains, for instance, gravel and rock are scarce as hen's teeth, so we tried to design a drainage system which would utilize the coarse material as efficiently as possible. The layout calls for a perforated culvert pipe, surrounded by sand and gravel filters, just downstream from the core of the dam. The pipe is sloped to an outlet in the spillway bucket. Obviously, it will be easy to measure the seepage collected in the pipe, either by installing a weir, or simply by using a couple of barrels and measuring the time required for filling. Whenever possible, some little trick like this should be planned in advance, so that subsequent measurements can be made without too much trouble.

In regard to the seepage through the embankment itself, I feel that we have gone about as far as we can with laboratory models, and that what we need now is verification of predicted flow nets by means of observation wells sunk into the finished structure. This work is not at all difficult nor expensive, yet there has been surprisingly little of it done. Flood control dams with open outlets, like the Miami dams, are not well adapted to such studies, because the water level does not stay up long enough to establish steady flow conditions, but whenever there is a substantial amount of storage, some attempt should be made to find out how the water is acting within the body of the dam, as well as in the foundation.

The efficiency of filters is an important element in controlling seepage. Personally, I am a great believer in filters and in adequate drainage systems generally. I think we can save a great deal of yardage in the downstream half of dams and the landside half of levees by intelligent use of underdrainage. I also believe that a properly designed and constructed filter will function indefinitely, although there are many engineers who would not agree on this point. We have not given as much attention to systematic and long-continued investigation of filters as we might have, but the situation is being remedied right now. The Little Rock District has already started tests, realizing that the safety of dams like Great Salt Plains depends primarily on correct functioning of the drainage system; also, programs of study are planned for the coming year, both at Harvard and at M. I. T.

I imagine that this discussion of some of the low spots in our knowledge may have sounded discouraging. I do not intend it to be discouraging; on the contrary, I think that a clear understanding of our weak points is in itself a hopeful sign. After all, when you stop to think about it, building a dam is a rude interference with a condition of natural equilibrium which has existed for many years. A peacefully flowing river is suddenly boosted out of its bed a hundred feet into the air, and conditions in the entire surrounding territory are turned topsy-turvy almost overnight. Banks and slopes which were stable are stable no longer; materials in the valley bottom are subjected to forces the like of which they had never before experienced; deeply buried layers which had little part in the scheme of things suddenly become important. And in making these radical changes we are dealing with materials of which our understanding is still very imperfect. It is not surprising that we find ourselves faced with many difficulties, and that occasionally we come a cropper. We should be thankful that we can do as well as we do, and hopeful that continued study will enable us to do a little better in the future.

MINUTES

of the

DISCUSSION OF A RESEARCH PROGRAM AND SPECIAL PROBLEMS

at the

SOILS AND FOUNDATIONS CONFERENCE

Held at the

United States Engineer Office, Boston, Mass.

June 20 & 21, 1938.

MINUTES
SOILS AND FOUNDATION CONFERENCE
Boston, Massachusetts
June 20 - 21, 1938

Captain James H. Stratton, Chairman, opened the conference at 9:30 A.M. by introducing guest consultants Doctor Rutledge of Purdue University, and Mr. Irving B. Crosby, Geologist. An abstract from letter of Brig. General M. C. Tyler, Assistant Chief of Engineers, in regard to this conference, was then read:

"My idea in promoting the Soils Conference was to obtain from the officers and engineers of the Department their recommendations on the investigations and research considered necessary in this field after joint discussion and careful consideration. Also I hope that such a conference will help to create a mutual understanding of the soil mechanics and foundation engineering problems among the officers, soil mechanics experts, design engineers, and geologists.

"The conference should in no way commit this office to any definite program, nor to the hiring of consultants other than for the duration of the Soils Conference. After a careful study by this office of the detail report of the conference it will decide just how much of the work can be done in the immediate future and where it should be performed to best serve the interests of the Engineer Department as a whole. Any specific investigations or research desired by a Division or District Office should be submitted to this office in the usual manner for approval."

The following general directives for conduct of the conference were then given by Captain Stratton:

"In order to give point to the discussion at this conference and to provide a basis for its initiation, a specific research and investigational program has been prepared and distributed to all District and Division delegates (letter of this office dated June 15, 1938). The specific program is based on the suggestions received and is intended to encompass the broadest possible program.

"The specific program is intended to form the basis for discussion and to provide the framework for elaboration at the conference. It is not intended that the use of the specific program shall limit or circumscribe either the discussions or the research program that the conferees shall recommend to the Chief of Engineers.

"It is proposed to conduct the discussion in the order given in the 'Specific Program', subject always to such departure as circumstances may warrant. It is desired that the discussion be kept within limits pertinent to the subject under consideration. By this I mean that the discussion of 'undisturbed sampling', for example, should not be intruded into the discussion of 'yielding foundations'.

"It is further proposed that the discussion period of each subject listed in the Specific Program be divided into two distinct phases. The first, or General Discussion Period, is open to everyone for such expression of opinion, theory, or recitation of experience, as is applicable. After the General Discussion Period, there will be a pause to permit the District and Division delegates to confer so that they may compare notes and arrive at specific suggestions or proposals for submission to the conference body. The second period will be given over to the specific suggestions for research to be submitted orally by the selected spokesman for each District and Division as proposed in our memorandum of June 17, 1938. Stenographic notes will be taken during the discussion period, and it is desired that the spokesmen speak slowly and distinctly, and that the proposals be as specific as the subject matter permits."

<u>MR. CROSBY</u>: I would like to make a few general remarks at this point. All Soil Mechanics studies and laboratory investigations of foundation problems of whatever kind are applied to geological formations and structures. Unless those formations and structures are correctly understood the most accurate laboratory work will be misleading. Therefore the proper relation between Soil Mechanics and Geology is very important, since without this relation Soil Mechanics will become one-sided and lose much of its practical value.

Soil Mechanics when applied to foundation problems deals with sedimentary deposits which are laid down in accordance with geological principles. A thorough understanding of these principles and extensive study of sedimentary deposits is essential for the correct interpretation of them. Except with the most simple conditions, the interpretation of these deposits can best be made by a practical geologist who has had extensive experience with them.

I believe that the most satisfactory relation of the two sciences is for the geologist to work out the underground conditions, to describe the different formations and the relations of their beds as accurately as possible, to estimate the probable degree of accuracy of his interpretation, and to estimate the worst possible conditions. Then the Soil Mechanics expert should apply the results of his studies and tests to the interpretation made by the geologist with the assurance that the final conclusions will be the most accurate possible.

Many investigators in Soil Mechanics admit the desirability of having a competent practical geologist investigate the underground conditions, but few put it into practice. Closer cooperation between specialists in Soil Mechanics and Geologists who have specialized in foundation problems will be of great advantage to both sciences.

* * * *

CAPTAIN STRATTON: We will proceed with the discussion of A (1), the stress-strain characteristics of cohesionless soils dividing this discussion into Purpose, Scope, and Procedure.

A. - STRESS-STRAIN CHARACTERISTICS

(1). - COHESIONLESS SOILS

Purpose: The design of earth structures and of structures resting on earth foundations, may be considered as a problem having two phases:

a. The structural and foundation stresses;

b. The resisting strength of materials in the foundations and in the structures. The investigation of the stress-strain characteristics of soils provides an approach to a better understanding of the second phase of the fundamental design problem and indirectly also of the first phase. (See Item D).

Scope: The determination of the stress-strain characteristics of various types and gradings of cohesionless soils at varying densities and loadings.

Procedure: Laboratory investigations by testing with the tri-axial compression machine, direct shear machine and consolidation apparatus. The investigation to encompass the development of the method and technique of the testing procedure.

DR. GILBOY: Would it be possible to take care of this cohesionless soils program by recommending that the investigations now being carried on by the Boston District be continued?

CAPTAIN STRATTON: Yes. I should like to say that the Boston District intends continuing its investigation on cohesionless soils since we have a number of dams to be constructed of this material. However, this investigation should not be limited to the Boston District. I want to remind you that there will be a tri-axial compression machine at the Vicksburg Waterways Experiment Station.

The Chairman then asked for a discussion on the scope and procedure as outlined for cohesionless soils. There being no comment, he requested that the delegates proceed with the discussion of A (2), Cohesive Soils.

A (2) - COHESIVE SOILS.

The same general comments under A (1) apply to this phase of the investigation of the stress-strain characteristics of soils.

MR. BUCHANAN: Recent research in the stress-strain characteristics of cohesionless materials indicates that much remains to be done. It appears that similar research should be performed regarding the same characteristics of cohesive materials, since both cohesionless and cohesive materials are used so extensively. Now is the logical time to plan such research. Who is to do this work? A decision regarding this matter is desirable.

CAPT. STRATTON: We are looking for volunteers.

MR. BUCHANAN: An excellent opportunity is afforded at Vicksburg for doing such work. One of the tri-axial shear machines constructed at Harvard is to be transferred to the Waterways Experiment Station at Vicksburg. An abundance of cohesive material exists within the Mississippi Valley, and this work could well be undertaken. A program for research along this line could be submitted for consideration.

CAPT. STRATTON: It should be submitted to the Chief of Engineers.

DR. CASAGRANDE: If a special research group is established at the Waterways Experiment Station, the handling of research problems would not interfere with the station's handling of the specific problems delegated to them.

LIEUTENANT DAVIS: To effect coordinated action through the Chief's Office, I recommend use of the already established Soil Mechanics Research Center of the U. S. Waterways Experiment Station. The urgency of specific problems has relegated pure research work to a position of low importance. The recommendations forthcoming from this conference should be the driving force which will properly emphasize the importance of research work. Also note that the Soils Research Center of the Waterways Experiment Station periodically issues Soils Research Bulletins, incorporating information of general interest to the entire Engineer Department, particularly information about specific problems having a common application.

DR. CASAGRANDE: It is not wise to divorce research men entirely from the application of research to specific problems. This is particularly true of the men in charge of research who should have plenty of time to do the thinking, and should be given plenty of opportunity to cooperate in the solution of difficult problems. To make this possible they must be aided by a sufficient number of competent assistants.

In talking about research we should keep in mind the two different types of research, both of which are needed. One type in which the practicing engineer is chiefly interested, deals with the solution of a given engineering problem. Such problems usually contain so many combined elements that it is not possible to wait until each factor is fully investigated; or else the theoretical interrelationships are so complicated that a direct solution is at present not possible. Therefore we attempt a solution e.g. by model tests. And if one approach fails, we try another simpler one, until some kind of results are obtained. Such work is research, to be sure, but of a crude nature. It is very important that in the presentation of the results of such work the limitations be clearly stated. Probably the answer will not consist of a single figure but of a wide range, and it may depend largely on the experience and judgment of the engineer carrying out the design, rather than on the men who carry out the research, to narrow down this range to a practical value.

The other type of research deals with the fundamentals of soil mechanics. It is this type of work for which the new research group in your Department should be created. The approach to fundamental research should be quite different from the approach to problems which the practicing engineers bring to us, which require some sort of an answer in a given time. It may require years of painstaking work before satisfactory results are obtained in such scientific research. While such work is in progress it does not seem to benefit directly any practical problems, and therefore such work has received little support in the past. Its real advancement was chiefly dependent on a few individuals who are doing it for the love of it. Another reason why progress in the study of the fundamentals is so slow is that this kind of work requires a special talent which is not abundant. Most of the men doing so-called research are anything but qualified for this work. It is this type of man who swamps the periodicals with publications of "new discoveries", which only waste the reader's time. The Engineer Department will have to be very careful in the selection of the men entrusted with this work if they wish to prevent much money being spent for worthless results.

CAPT. KRAMER: We are discussing the investigation of stress-strain characteristics of cohesive soils, with particular reference to the application of the recently developed tri-axial loading machine. The consultants say new techniques give rise to certain doubts and present new avenues of approach in the investigation of shear characteristics. For cohesionless soils, the Boston District has charted a new road. However, it is with clay soils that most of our districts are concerned. The work with cohesionless soils has shaped the problem of investigation on clay soils. Mr. Buchanan says he is to get one of these triaxial machines at Vicksburg. I recommend that we delegate to the U. S. Waterways Experiment Station the investigation of cohesive soils.

MR. BUCHANAN: I concur with Dr. Casagrande in his general plan. It is obvious that we should not devote our entire attention to pure research and forget the application of this work to real problems, for thus we would lose interest and become unbalanced. A unique situation prevails at the U. S. Waterways Experiment Station, in that a great variety of problems are presented to this Station by all District Offices. Thus, a Research Division in the Soil Mechanics Research Center would have the opportunity of working occasionally on a specific job, thereby maintaining interest and enthusiasm on a high plane.

CAPT. KRAMER: From Dr. Casagrande's explanation of how he budgets his time, and considering that under the stress of a great many extraneous influences he has been able to develop his new machine, it is only a question of management on the part of the Experiment Station to conduct this research program.

COL. PARK: The Waterways Experiment Station for some time has been doing all of the experimental work for the Mobile District. I sincerely hope that in the future we are not going to send to the Station for the solution of our specific problems and find there only the knowledge gained from the relatively few experiments in specific problems. The Mobile District wants to see a general research center established at Vicksburg with proper personnel, adequately financed, as well as research on specific problems.

COL. BESSON: The Galveston District seconds the remarks of Col. Park.

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CAPT. STRATTON: We will then proceed to discussion of Item B.

B. - DEVELOPMENT OF EQUIPMENT FOR FIELD OBSERVATIONS.

Purpose: To further the knowledge and understanding of the behavior of embankments and foundations, giving consideration to undisturbed sampling of soils and observations of seepage, settlement, deformation and stress distribution.

Scope: Development of:
(a). Boring and sampling equipment
(b). Piezometers
(c). Settlement gages
(d). Pressure cells

Procedure: A fact finding survey to determine the value of equipment now in use; development of apparatus based on the experience gained to date.

We will limit the discussion to the specific problem. We will have general discussion first. You are all free to ask the consultants questions. They are here for your benefit.

MR. CUMMINS: We have made over three hundred thousand feet of test borings at Fort Peck, in which we have attempted to take undisturbed drive samples, and have developed some very efficient sampling equipment, and various types of samplers. Several of the different districts have requested information on this equipment, and we have this information available.

CAPT. STRATTON: We have done much sampling in the Boston District on cohesionless soil below the water line. A report will also be available for distribution in a few months.

DR. GILBOY: I want to call attention to the procedure listed under Item B. I think we could dispose of this subject by recommending that such a fact-finding survey be instituted and that the various districts working on these problems submit at least a brief report on what they have been doing, for further guidance.

MR. BUCHANAN: I agree with Dr. Gilboy. We could accomplish much. As a matter of fact, we have been very much concerned with the problem of sampling. We offer the research bulletin as an outlet through which this information can be sent to you. A record of the experience of the various districts could be assembled and distributed through this agency.

MR. HANSEN: Mr. Cummins has brought out the fact that they had considerable equipment for sampling below the water line. The Little Rock District has used this equipment with very few additions and has obtained excellent results. These results have eliminated at least half the test pits and will, in the future, eliminate most of the test pits in cohesionless soil.

MR. BLOOR: In the Ohio River Division we have recently completed 13 earth dams in Muskingum basin and are now engaged in building a number of levees along the Ohio River. We have been trying to promote a series of field measurements following the completion of the Zanesville dams in order to establish whether or not our basis of design is very accurate. This will necessitate development of some of this equipment. I am going to try to get Departmental approval for such a program on these Zanesville dams and possibly on some of our levees, I suggest the Zanesville District as a good place for the development of this equipment.

MAJOR SHAW: I concur with Dr. Gilboy. I think the Engineer Corps as a whole is losing a golden opportunity. We are cooperating with Vicksburg very closely, and whenever we have to decide on a piece of equipment or a method we get either Lieut. Davis or Mr. Buchanan over, and talk over the possibilities of using it. We think that necessity is the mother of these discoveries of equipment. Each development comes about as the necessity for it arises. When we find a system that works, we use it. If all the material were sent to some central clearing station like Vicksburg to be passed out to the Corps as general information, I think we could get a great deal out of it; as the problems came up they would go out to people in other districts who might have the answers right there. With all districts having their own problems and their laboratories, they answer their own problems, but nobody else knows about it. They make their report, which in turn is included in a report of 12 or 14 thousand pages, and other districts do not have the time to read through all that material. I suggest that we cooperate more closely and let the necessity of the situation develop the new types of instruments and methods used, and then disseminate that knowledge through the Department.

MR. FLACH: At Conchas Dam we have built a test dam one-fifth the size of our high south dike. This test dam has special equipment which consists of plain and perforated pipes, Goldbeck cells for earth pressure, hydrostatic pressure cells for water pressure, and subsidence plates for settlement observations. The seepage lines have not been fully developed. We are taking periodical observations and will write a final report in connection with our first report on construction and equipment.

MR. SLICHTER: The sampling equipment which Mr. Cummins described was developed at Fort Peck and was recently used in the underground explorations at the Marshall Creek Dam, where it has not proved satisfactory. For sampling during the reconstruction of the dam, we have designed an undisturbed sampler which is driven into the ground by a hydraulic ram. We plan to check the efficiency of this sampler by comparing the test results with those of samples taken later by hand at the same location during the excavation of the foundation soils. The sampler and accessory equipment is now under construction and will be placed in operation within the next 30 days.

MR. BUCHANAN: The U. S. Waterways Experiment Station has recently used a new type of drilling machine and excellent results were obtained. The George Failing Supply Company of Enid, Oklahoma, developed the machine, which is very speedy and satisfactory. The machine is mounted on a truck and is very mobile. The motor of the truck drives the drilling mechanism. It can drill to a depth of approximately 2,000 feet. It has a collapsible derrick, which, when in position, can handle tools or drill rods 25 feet in length, without difficulty. The drilling is done by a simple rotary mechanism, which is provided with an arrangement of hydraulic rams for pushing the sampling tube into the material, rather than driving it with a hammer. A winch capable of lifting 20,000 pounds (single line pull) is provided with the machine, as well as a pump for handling both drilling muds and clear water. The machine is not only suitable for rapid drilling through soil, but also through rock, and uses any type of rotary bits, diamond, fish-tail, auger, or such special bits as the "Hughes" rotary bit. I recommend it for all general drilling and exploration work.

CAPT. STRATTON: We have perfected some of the methods which have been developed in other districts, to make the sampling tests at Franklin Falls. Would anyone like to elaborate on the technical phases of that type of sampling?

DR. GILBOY: Should we spend any more time on this subject? I think that all of the districts have had a good deal of the same experience in this matter. It seems to me that we should decide on some plan by which we can find out what has been done by other agencies.

DR. CASAGRANDE: I suggest that the Engineer Department collect all their information on boring and sampling methods and turn it over to Dr. Hvorslev so that he may include it in his report to the American Society of Civil Engineers. I believe that we should wait before making further detailed recommendations until Dr. Hvorslev's report is available.

CAPTAIN STRATTON: The Boston District would like to be considered for the elaboration on investigations on boring and sampling equipment for sampling cohesionless soils. I will be glad to hear more specific proposals.

MAJOR NOLD: The Binghamton District wishes to be considered for inclusion on items A, C and D of development of equipment for field observations. We have to do some of it anyhow. My assistants will elaborate on this very briefly.

MR. HOUGH: We have many ideas on paper about boring and sampling equipment. This conference brings out the desirability of going ahead with some of these things and I would like to know what districts are actually going to construct some of this equipment. Has anyone here tried out equipment used by telephone companies for setting telephone poles, for borrow areas where rapid sampling is desired at shallow depths? On settlement gages my interest in cells has been intense because at Cornell University, where our laboratory is located, one of the graduate students is doing thesis work on pressure cells and he has some good ideas. Some of his trouble is with the construction of the cells. The Binghamton District should cooperate with this and arrive at a conclusion which would be useful to the whole department.

MR. SLICHTER: No doubt there is considerable information on the use of various types of sampling equipment which would be valuable to anyone contemplating underground explorations. The reconstruction of Marshall Creek Dam offers an excellent opportunity for the comparison of samples taken with sampling equipment with undisturbed samples taken in open excavation areas. As a practical research problem, I recommend that the districts which have developed special sampling equipment and are interested in checking it against undisturbed test pit samples during the Marshall Creek Dam reconstruction should make a specific request to the Chief of Engineers for the authority. The efficiency of each sampler could be evaluated by comparing samples according to the method used by Dr. Hvorslev.

MR. BUCHANAN: In answer to the question raised by Mr. Hough, I recommend a new machine which is very suitable for rapid exploration of borrow pit areas. This machine is designed to work on a pick-up truck and operates in the same general manner as similar heavy duty machines used by telephone companies. It can bore to approximately 15 feet in a very few minutes. It has actually been used on projects and has proven satisfactory.

MR. WELLS: I recommend that the scope of item B be enlarged to include the development of apparatus and methods for field determination of foundation permeability. At present, we attempt to determine flow patterns in pervious foundations by taking large numbers of undisturbed samples (if possible) and testing each sample in the laboratory. We can determine the permeability of each sample very accurately. However, in most natural soil deposits, variations in permeability are tremendous and, consequently, large errors may enter in the interpretation of our laboratory results. The only way to overcome this difficulty is to test the pervious foundation, as a whole, in the field. The problem resolves into development of apparatus for doing this.

DR. CASAGRANDE: For years I have been interested in testing the permeability of the ground by pumping out observation wells, or feeding water into wells, and then observing the changes in elevation of the surrounding ground water table. I have come to the conclusion that this method is practical only for relatively uniform soil conditions. Where the soil conditions are very irregular and contain very porous sections, this method is difficult and expensive to apply. This method, however, did prove successful, in spite of irregular glacial deposits, on the Quabbin project in Massachusetts, where pneumatic test caissons were used as wells, and continued pumping for many months effected a substantial lowering of the surrounding ground water table.

MR. HOUGH: I think this conference can recommend development of apparatus for the field measurement of the permeability of soils in place. I recommend that this be done.

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CAPTAIN STRATTON: We will now proceed to discussion of Item C (1).

CAPTAIN HILL: We have an immediate problem of levees and there is an extremely non-homogenous material which varies along the levee. To put that many tests through the laboratory is impossible. We have tried the Proctor needle and it does not work for us at all. Is there some mechanical means whereby we could get the same results as with the Proctor needle?

DR. GILBOY: I do not understand the problem you are up against. Are you trying to measure the hardness or softness?

CAPTAIN HILL: We are trying to get the consistency of the levee material, a measure of its proper classification in order to determine if the material from the borrow pits is satisfactory for actual construction.

DR. GILBOY: I do not know. I should have to think that over.

CAPTAIN HILL: The immediate problem is one of field observations. We want to eliminate certain tests because it is not feasible to deal with them in the laboratory. We must get some measure of the stability of the material as we go along. There is a question in my mind if some mechanical device such as the Proctor needle could be used to arrive at some reasonably consistent result, so that we can discard or use the material from different borrow areas.

DR. GILBOY: I do not know of any such method. That is a special problem which your district might investigate.

CAPTAIN HILL: Is any such an investigation being carried out at the present time?

DR. CASAGRANDE: It is not yet possible to determine in the field the suitability of a material for dam construction. This can only be determined from a reliable knowledge of the stress-strain relations and permeability of the available soils. Measurement of the consistency, either in the laboratory or in the field, does not yield the stress-strain relations and permeability of the material, but tells you only the consistency in which this material happens to be. By changing the water content, one can change the consistency radically, and yet the soil is still the same, either suitable or unsuitable for dam construction. The consistency is important in connection with proper construction after it has been decided on the basis of other investigations that a material is suitable, and after a proper cross section of the dam was designed.

MR. TORPEN: Any inspector will find that the human eye and a little practical judgment is all the equipment necessary for this procedure.

CAPTAIN KRAMER: Assuming we have found by laboratory tests some definition of what constitutes rejectable material, the application is not a mechanical problem involving needles but involving human judgment. Go down to the Lower Mississippi Valley where they have been building levees for a long time and look for an inspector with web feet and calloused hands. That is probably the best mechanism to serve your purpose.

DR. CASAGRANDE: I have found in my work with highway departments that it is possible to train an intelligent young man to identify a limited number of soil characteristics in the field. I believe that after thorough study of the materials which will be used in a dam, it should be possible to train men to identify these soils in the field with their own hands and a few very simple field tests, even if the same soil may appear in widely different consistencies.

MR. CUMMINS: At first we ran mechanical analyses in the laboratory on all borrow pit samples. Later we found that trained inspectors became so familiar with the material that this was unnecessary. We established a field classification office and have a couple of men there who can give us a very accurate analysis and can classify several hundred samples a week. Of course, we maintain a certain amount of laboratory control over the field classification.

CAPTAIN STRATTON: We are looking for specific proposals and recommendations on Item C (1).

C (1) - SEEPAGE THROUGH EMBANKMENTS AND FOUNDATIONS.

Purpose: To further the knowledge and understanding of seepage in embankments and foundations.

Scope: (a). Comprehensive review of all recent experimental and theoretical work on the seepage through earth dams and foundations, either in published form or in reports, and by interviewing those with experience in such work, for the purpose of establishing a clear picture of the relative merits of the various methods and for establishing the type of problems for which laboratory results are still required, indicating the best approach to their solution.

(b). Field observations on temporary and permanent water retaining structures for the purpose of establishing the deviations which may be expected from results of theoretical or experimental investigations due to "minor geologic details".

Procedure: (a). Fact finding survey by selected qualified personnel.

(b). Observation by means of permanent observation wells and open-ended pipes jetted down to various elevations for rapidly establishing pressure profiles. Temporary structures, such as earth cofferdams, lend themselves for the purpose of learning the possible deviations from results obtained on the basis of simplified assumptions.

MR. BUCHANAN: Much interesting information regarding the seepage through a dam 30 feet high has been assembled by the U. S. Waterways Experiment Station. The dam was designed and its construction supervised by the staff of the Soil Laboratory of this Station. Two types of observation wells and approximately 18 of the Goldbeck pressure cells were installed. The total head is approximately 25 feet, and was maintained with plus or minus 0.5 ft. during the period of observation. The results obtained from the observation wells have been gratifying. The Goldbeck pressure cells have failed to function satisfactorily. Approximately 11 months' time was required for the seepage to develop fully through this structure. The best type of observation well was found to be a closed pipe extending down to the desired position with a filter at its bottom. The perforated type of well has not proven satisfactory. The water level in the pipe can be determined accurately to within .01 of a foot, with a simple sounding device. Following the construction of the structure, additional wells have been placed and have functioned satisfactorily, indicating that such wells may be placed in completed structures.

MR. PHILIPPE: I have a specific recommendation. In the Muskingum district 13 earth dams of a variety of shapes, sizes and heights have been built. On many of these exhaustive studies particularly as to seepage have been made. It is also possible on most of these to collect and measure seepage, drill various embankments and set up methods of measuring pressure. I recommend very strongly that this be done.

DR. GILBOY: I want to indorse Mr. Buchanan's recommendation and point out that although we can sink observation wells into existing dams, it does not open up such a wide field, because in order to know whether the observations correlate with preliminary studies we must have made some preliminary studies. There are a lot of dams where very little is known about the interior properties. We should check up on cases like those in the Muskingum district where a great deal of information was obtained, and in dams currently being built we should put observation wells so that we will have something to correlate seepage studies against.

MR. SENOUR: The rate at which seepage develops should be investigated. Particularly in levees on streams having floods of brief duration, a great deal of money could be saved by reduction in levee cross section if we knew how rapidly seepage develops.

MR. PHILIPPE: In the construction of levees at Lawrenceburg, Indiana, they are contemplating the installation of such wells.

MR. FLACH: Investigation of seepage through structures using both plain and perforated pipes is necessary for this reason: if you put a plain pipe at a certain point you read the pressure at that point. If you have a weak lens you may still have seepage through that lens and not notice it. A perforated pipe would counteract these weak points. A combination of both types of pipes is necessary.

MR. GRIFFIN: We have what might be termed two types of laboratory field work in connection with seepage. On many navigation dams in the Upper Mississippi River we have installed some means of measuring uplift pressures. Pipes with screened openings near their bottom ends have been provided beneath the masonry piers, sills and aprons. By measuring the static water levels in these pipes it will be possible to approximate the flow nets and seepage beneath the structures. These dams have heads which range from 5.5 to about 25 feet. These are founded on sand and constructed with steel sheet pile cut-offs on both the upstream and downstream sides. We are also investigating the seepage into levee districts, which exist for a considerable stretch along those portions of the Illinois and Mississippi Rivers now being improved for navigation. The matter of seepage into several of these districts is important in view of the damage claims which may arise. Pipes are being installed for measurement purposes. Although the differences in head will be low, this work will be of importance in connection with proposed studies.

MR. BUCHANAN: I concur fully with the comments of Dr. Gilboy and believe that we should place observation wells in structures suited for this purpose, particularly those formed of homogeneous material. Such a series of studies would permit great improvement in our present methods of design regarding seepage. The investigation of structures in which different kinds of materials of varying permeability occur would yield little information which could be analyzed by present methods, for too many variables would have to be considered in the study of such a dam. In answer to the question raised by Mr. Flach regarding perforated wells - we have found that they clog badly, and in general are unsatisfactory.

MR. CROSBY: Laboratory results of seepage through foundations must be applied to underground conditions with great caution on account of the frequent irregularity and variability of those conditions. Borings may appear to indicate the underground conditions satisfactorily but in many cases conclusions drawn from them without a thorough knowledge of the stratigraphy of the different types of deposits may be misleading. Some types of deposits characteristically have lenses, tongues and branching strata while other types are more regular in their stratification. No reasonable number of borings can prove whether a bed is continuous between two holes or whether the holes struck two lenses, but recognition of the type of deposit will indicate the probabilities. Discontinuous and variable strata, tongues and lenses are most common in some types of glacial outwash deposits and in flood plain deposits, but some glacial deposits are very uniform. Regular stratification is common in some lake and sea deposits, but there are exceptions which can be detected by geological study. A geologist experienced in sedimentary deposits can identify the type of deposit, know the type of irregularity to expect, make the most accurate interpretation possible of the boring and other data, and estimate the probable degree of accuracy of the conclusions.

The following procedure is recommended in applying results of research investigations to seepage through embankments:

1. The underground conditions should be determined as accurately as possible by a competent, practical geologist. If conditions are simple, the results of laboratory tests can be applied to them, or they can be reproduced in a model.

2. If underground conditions are moderately complex it may be possible to estimate an average condition and apply the laboratory results to this.

3. If underground conditions are very complex a most careful study of the stratification must be made and the worst possible conditions determined and the laboratory results applied to them, unless some method of field test can be devised which will automatically evaluate the underground complexities.

Field permeability tests merit further investigation as they may provide the most satisfactory method of determining seepage through complex deposits.

Indiscriminate application of laboratory results to underground conditions which may be complex is very dangerous.

In studying seepage through embankments, attention must be given to the effects of stratification and the parallel arrangement of flat grains. If the embankment is placed by a hydraulic method, conditions somewhat similar to natural deposits will occur and tongues of pervious material may extend far into the impervious core.

MR. HOUGH: Relative to seepage measurement, Mr. Harza of the American Society is now working on a device for measuring seepage pressures. When we make our recommendations for development of equipment we should consider Mr. Harza's work.

DR. GILBOY: I concur with Mr. Crosby's remarks on geological studies. We soil mechanics, when trying to design a dam, ask a geologist all sorts of questions about stratification, history of deposits, etc., for it is difficult to evaluate the situation without such knowledge.

MR. SLICHTER: I recommend that a study be made of the seepage through all of the existing dams in the Department, including the Fort Peck Dam. For the study of seepage, the Fort Peck Dam probably has an advantage over flood control reservoirs such as those in the Muskingum Valley because the pool level will be held at a fairly high level. Reservoir operations for navigation will produce a fairly uniform fluctuation of pool level during each yearly cycle. A very complete record of the soils in the dam has been made and seepage pressure pipes have been installed.

MAJOR SHAW: Has any district or division made a seepage test of the conditions of the foundation both prior to and after construction of the dam, and what process did they use?

MR. PHILIPPE: In the Muskingum District we made permeability determinations both by laboratory methods and then later, by excavating test pits and pumping from them. There was reasonable agreement. Furthermore, we have approximately measured the seepage after the dams have had water against them. At the Mohawk Dam the seepage was very much in line with our estimates made previous to the construction of the dam. Both foundations and dam were considered in this particular case because 95 per cent of the seepage goes through the foundations.

CAPTAIN KRAMER: We have at Conchas a case of impervious foundation in a test dam which is a step larger than a flume test. This dam will fit Dr. Gilboy's specifications as to prior information.

DR. CASAGRANDE: I do not wish to discourage observations of water levels within dams. It is much more important that such measurements be made in pervious foundations of dams. If a dam was thoroughly analyzed and properly built, the line of seepage will for practical purposes not deviate seriously from the theoretical determination. However, the irregularities of a natural soil deposit are such that the seepage through a foundation may differ radically from the flow nets determined from a few permeability profiles.

MR. TORPEN: At Bonneville we had the possibility of determining seepage through the dam. Under the direction of Mr. Harza, consultant, we made several pumping tests, sank some drill holes, made mechanical analyses of materials, and determined coefficients of seepage. We correlated all these matters. After building a cofferdam for the powerhouse and pumping out the inclosure of 25 acres we estimated seepage to be about 25 to 50 c.f.s. per second which was very close to the predicted amount. This seepage was subsequently reduced to 17 c.f.s. per second. We believe it is possible to determine the coefficient of seepage within practical limits from a study of mechanical analysis, pumping tests and through the flow net method. For the dam proper, which was founded on bedrock and the abutments of which were only against terrace deposits, the question was how far to put cut-off walls. There were many recommendations for cut-off walls from 1,000 feet to 1 mile in length, costing from one to ten million dollars. After a very thorough study of this we concluded that the seepage would be a relatively small amount. This experience at Bonneville shows what thorough study can do.

MR. SENOUR: The Mississippi River Commission has made many observations on seepage through levees during floods and I recommend that the practice be continued wherever there are floods against levees. Observation of seepage should be made when the levee material is fairly homogeneous or where variation of material is known. Some of our earlier observations were disappointing because non-uniformity in the materials caused such erratic variations in the seepage line that little could be learned.

DR. GILBOY: If possible, I recommend that the procedure as outlined in the memorandum be accepted and that such districts and divisions as wish to cooperate in that program should signify that desire.

MR. PERSONS: The Conchas Dam South Dike is an example of rolled-fill on impervious foundation. We have worked with a number of different scaled models and are now observing the action of an 18-foot high test dam. The 90-foot South Dike will have in it a number of hydrostatic pressure cells so that a check of its seepage line against those in our model and test dams will be possible. We recommend that the results of our studies be included in your studies of seepage through embankments and foundations.

MR. HOUGH: I propose that we insert in this section the suggestion that in all earth works which the Department constructs, we should not only insert equipment for field measurements but also to obtain samples of the materials at frequent intervals as the works are constructed. I suggest these samples be subjected to a very careful analysis and a record made of the date, so that after completion, we will have the necessary information to make whatever studies may be desired.

MR. PHILIPPE: The Ohio River Division recommends that the Zanesville dams be observed. These dams included the conditions where structures were built on impervious foundations as well as pervious foundations. Our geologist suggests that permeability studies be made by the study of underground flow, by observing the movement of salt introduced in one well-point to another well-point. This is commonly referred to as the "Slichter method". The installation of gages and pressure reading equipment sould be promoted by the Corps of Engineers, especially in flood walls and levees.

MR BUCHANAN: The numerous structures proposed in the Mobile District offer many opportunities to observe the seepage through, beneath and around numerous types of engineering structures. I recommend that provision be made in the design of these structures for making desired observations.

COL. PARK: It is very important that all the very extensive data that the Vicksburg Laboratory is searching for be disseminated throughout the Department. We found we could dispense with elaborate and expensive foundations when the laboratory advised us about cohesive soils.

MR. JERVIS: A most complicated situation in the study of seepage is where seepage through the section and the foundation is intermingled. This introduces certain difficulties in computing where the topmost flow line will be and what the pressures will be, particularly around an opening between the pervious foundation and a levee section itself. The levees in the Vicksburg District are founded on pervious materials which are covered by impervious material so that foundation and levee seepage can be studied separately. Such study is important in order to get fundamental facts in simple form.

MR. GRIFFIN: In the Upper Mississippi Valley Division we have some possibility for seepage studies. We do not have quite as much head as at other locations, but we do have more constant heads than those in effect at some flood control dams during the passage of floods. In connection with our damage claims we contemplate some seepage studies. In addition we have facilities for the approximation of seepage beneath certain navigation dams. These should be included in the program.

MR. HARTMAN: I suggest that in the course of construction of various structures we set up a fund so that we can guarantee that all suggestions made for these dams be carried out.

AFTERNOON SESSION

June 20, 1938.

CAPTAIN STRATTON: We will resume the conference with a consideration of items C (2) (a) and (b) "Effect of Discharge of Seeping Water on the Stability of Embankment Slopes".

C (2). - (a). EFFECT OF DISCHARGE OF SEEPING WATER ON THE STABILITY OF EMBANKMENT SLOPES.

Purpose: By systematic investigations to ascertain the effect of seepage on the stability of embankments.

Scope: Laboratory investigation of the effect of seepage on the stability of undrained downstream embankment slopes by the use of models constructed in glass flumes. Observations of behavior of model under severe conditions of drawdown.

Procedure: Embankment sections to be constructed of homogeneous materials of various gradings at various densities and ranging from cohesionless to slightly cohesive. Various sections of embankment to be used and conditions of test to include observations at various heads with and without tailwater.

MR. ACKERMAN: In connection with a study of cohesionless materials the Boston District should attempt to determine the laws which govern the erosion at the toe of a dam from which water is emerging. Little is known of these conditions. In the Middle West many sand dams have been constructed, and in certain cases it was cheaper to build the sections large and avoid the use of filters.

MR. HOUGH: As I see it the scope of C (2) (a) has been limited to embankments which have no drains in the slopes.

CAPTAIN STRATTON: We are considering this largely for structures that have no filter materials available.

MR. HOUGH: I should think that C (2) (a) might be amplified to include structures that do have drains.

DR. GILBOY: That is just a special problem as it is set forth here, the idea being to try to find out what effects occur at the toe of the slope which is not drained and how those effects are governed by deposits within. In certain cases adequate drainage is not feasible. This program would give us some information as to what we can do without drains.

MR. HOUGH: I don't want to minimize the importance of investigating undrained slopes. I simply propose to make provision for other types of slopes as well.

DR. CASAGRANDE: It seems to me that all other problems concerning structures with proper drainage arrangements belong under the heading "seepage".

DR. GILBOY: If I interpret your remarks correctly you believe that some provision should be made for study of the seepage forces in the downstream parts of embankments which are provided with adequate filters, and the effect of these seepage forces on stability. I think that would come under Section E.

MR. HOUGH: I wish to know whether or not it would be proper to consider here the subject of the effect of drawdown on slopes of homogeneous sections like levees.

CAPTAIN STRATTON: It is recommended that the last sentence under "Procedure", Item C (2) (a) be added after the last sentence in "Scope" and that the title of this item be changed to read

"Effect of Discharge of Seeping Water on the Stability of Embankment Slopes". We will proceed to Item C (2) (b).

C (2). - (b). <u>EFFECT OF SEEPAGE ON THE STABILITY OF FOUNDATIONS</u>.

<u>Purpose</u>: To ascertain the effect of seepage on the stability of embankment foundations.

<u>Scope</u>: Study of underground erosion in stratified soils and possible cavity formation and piping in foundations. Determination of the value of cut-offs and of lengthening seepage paths by use of blankets and aprons.

<u>Procedure</u>: Field investigation of existing structures and laboratory investigation by use of models.

<u>DR. GILBOY</u>: This whole problem, as you can see from the Scope and Procedure, is stated only in very general terms here. I would welcome suggestions from any of the members on more specific recommendations and study.

<u>MR. PHILIPPE:</u> I should like to see these studies extended specifically to abutments and abutment contacts.

<u>CAPTAIN STRATTON</u>: Are there any specific recommendations on Item C (2) (b) ?

<u>MR. BUCHANAN</u>: The levees in the Lower Mississippi Valley offer ample opportunity for the field studies desired. Any number of combinations of materials exist both in the foundations and structures. I recommend that the work be undertaken by the Districts in the Lower Mississippi Valley, either by observations of seepage through and beneath the levees, or by means of small scale model studies. Such work could also be undertaken by the Soil Mechanics Laboratory of the U. S. Waterways Experiment Station.

<u>DR. CASAGRANDE</u>: We particularly had in mind underground erosion beneath rigid structures such as concrete overflow dams. There are two phenomena which lead to the formation of cavities, gradual underground erosion, and finally piping. The first one was pointed out by Terzaghi many years ago and occurs in stratified deposits. Under certain combinations of fine and coarse grained layers, large seepage forces may wash particles from the fine-grained layer into the pores of the adjacent coarse-grained material, thus leaving cavities open which facilitate the flow of water, which in turn aggravates the cavity formation until the seepage pressures on the downstream side are increased to the point where boils appear and piping may occur.

More recently we have come to realize that there is another cause why fine-grained sandy soils are particularly susceptible foundation materials. Such soils are usually in a very loose condition and a disturbance may produce local internal subsidences and the formation of cavities below the base of the dam. Unless cut-off walls prevent the formation of a continuous channel, such a condition may lead to failure by piping. I believe that many piping failures of overflow dams are due to such internal subsidence of loose, fine, sandy foundations.

CAPTAIN KRAMER: One form of this phenomena occurs behind levees during highwater. It is known as "sand boils". Various kinds of "porous plasters" have been prescribed by many well-meaning doctors. The usual field treatment consists of equalizing the head locally until the water ceases flowing. It is very disturbing to see this material washing out from under a levee when the cause and effect are unknown. The real practical problem presented by this topic is: 'What is a sand boil?'

DR. CASAGRANDE: The problem which Captain Kramer has just mentioned is an important one but does not come exactly under this heading, unless the sand boils are a secondary effect of prior cavity formation. Many sand boils are simply caused by excessive upward seepage pressures on the downstream side of the structure. For relatively uniform soil conditions flow net studies should permit the design of dams and levees such that the formation of sand boils will not occur. Where the soil conditions are very irregular, some "minor geologic detail" may cause the formation of a sand boil in spite of careful foundation studies and conservative design of the structure. It will be the subject of laboratory and field experiments to determine in what form, by drainage wells or other means, one can cure ordinary sand boils.

MR. CROSBY: I would like to emphasize that we must learn whatever we can about underground conditions. Where you have very complicated deposits you can't perhaps forecast all of the conditions. You can't say exactly you have conditions that are going to cause sand boils or will result in undermining of foundations. In most cases you can point out that you have conditions where that is a possibility and you must look out for that and provide for it.

MR. PHILIPPE: Dr. Casagrande, can you suggest any form of field investigation which will determine the effects of submerged piping?

DR. CASAGRANDE: I believe that before we can make any definite proposal for a field investigation we have to feel our way into the problem in the laboratory to a certain extent. It is a very difficult problem.

MAJOR SMITH: During the last flood at Cairo, Illinois, there were many sand boils but the water from these upon analysis was found to be different from the river water. This indicates that the boils were caused by an interruption of the ground water regimen rather than by water coming through the levee.

MR. CROSBY: This may have been ground water which was being forced out through the added load caused by the flood. If you had an artesian aquifer or some similar artesian condition and added this load on it, you would force the ground water out.

MR. SENOUR: When boils work toward the levee and put out large quantities of sand, we spend large sums in emergency work, so we can't afford to overlook this very practical problem. It is important that we ascertain their origin, and effect or seek better and less costly means for their prevention or control.

DR. GILBOY: I think that there is possibly some confusion here with regard to just what we had intended to include in underground erosion. We were thinking of rigid structures such as spillways founded on soil rather than rock. We wanted to find out if any districts were interested in this particular problem.

COL. FLEMING: The St. Paul District and the Upper Mississippi Division are very much interested in this. We have rigid structures there and are measuring pressures, but not flow. It is possible that we might be able to devise some means of measuring flow.

MR. PHILIPPE: The Ohio River Division is contemplating the construction of flood walls which are rigid structures, on sand or other foundations, and the same problem is involved.

CAPT. STRATTON: From work which has been done in Germany on sand boils we have learned a lot about ground water and how to reduce this condition.

DR. CASAGRANDE: I suggest specific inclusion of the problem of sand boils along levees. Research on the problem of formation of cavities in the foundation would better be confined to rigid structures.

I agree with Captain Kramer that the use of simple open-ended pipes represents an excellent method for studying these problems. If we cannot observe directly what is going on in the subsoil, diligent and numerous measurements of water pressures may give us a clue as to what is happening.

MR. SLICHTER: The Missouri River Division is interested in this problem particularly with flood walls when supported on foundation piling or directly on the soil.

CAPT. STRATTON: With rigid structures on sand, it isn't a question of vertical versus horizontal pressures.

MR. BUCHANAN: In the Lower Mississippi Valley many impervious structures on pervious foundations have been constructed. I would like an opinion from Dr. Casagrande regarding the desirability of observations of seepage and the distribution of hydrostatic pressures beneath such structures. The data thus obtained would form the basis for further detailed studies of piping under the rigid type of structure.

DR. CASAGRANDE: Yes, this is a very good example where such investigations should be conducted. It is inevitable that in such investigations we shall collect information which will serve for the solution of several different problems. Many of these problems cannot be separated distinctly. They overlap or blend together.

MR. MIDDLEBROOKS: Rather than undertaking too much laboratory work, we can observe seepage changes on actual structures.

MR. BLOOR: Since these problems mean a change in the material, would it be possible to take samples of the materials by borings over a period of years? We expect to have in our flood walls in the Ohio River Division many cases of unusually homogeneous materials and if the problems under these walls develop dangerous proportions, you would be able to notice a change in materials from one side to the other in the face of the wall.

DR. CASAGRANDE: Sampling would give us information only when the soil is very homogeneous in character. However, in such soils, we should not expect underground erosion. In other types of soils it would be extremely difficult, if not impossible, to detect changes by means of sampling.

MR. CROSBY: The most dangerous condition is where you have thin laminations of fine silt in plain material, and the movement of the silt is the beginning of the trouble. You could not forecast this condition by taking samples and studying them.

MR. PHILIPPE: I suggest that the Corps study this problem both in the laboratory and in the field.

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CAPT. STRATTON: We are now ready to proceed with general discussion of the design of filters, Item C (3).

C (3). - DESIGN OF FILTERS.

Purpose: To develop principles of design of filters and filter wells so as to provide adequate drainage, prevent erosion of foundation material and the formation of cavities, and eliminate the danger of piping.

Scope: Determination of the required grading and thickness of filters, and grading and size of filter wells, including the range of influence of the latter on various types of pervious foundations.

Procedure: (a). Laboratory investigation.

(b). Field investigations by the installation of filters and filter wells of selected dimension, grading, etc., in levees which are known to offer problems involving foundation drainage.

MR. CUMMINS: I suggest that the scope of the investigation on this subject be enlarged to include all types of drainage systems, including subdrainage tile and perforated types.

DR. GILBOY: We should find out something about the required gradings and thicknesses of filter materials for use in drainage systems, particularly in such places as the downstream toes of dams. We are designing the downstream toes with the provision that we have several different layers of filter materials. We figure material of a certain size won't go through the pores of a material of another size, etc. Up to now, this has been a matter of individual judgment. Filters are not being used as often as they might, largely because there is some doubt as to their certainty of operation. If we can get some information regarding a correct design which will provide for certain operation over a long period of years, a great many objections will be overcome. In connection with sand boils, if you could intercept that flow at some point underneath the levee where there is weight over it to hold it down, you wouldn't get those sand boils. This is not being done because frequently you don't have a large amount of filter material available. We should develop and test some simple type, such as pipes surrounded by a small amount of filter material. Dr. Casagrande has already planned investigation of filters at Harvard University and investigation is being planned at the Massachusetts Institute of Technology, and I am sure both will be glad to acquaint the Engineer Corps with results as rapidly as we get them.

MR. BUCHANAN: A similar study is in progress at the U. S. Waterways Experiment Station. It involves the determination of relative effectiveness of drainage wells. The wells are placed on the landside of the levees through impervious blankets to relieve the uplifting effect of the hydrostatic pressure acting along the bottom of an impervious surface stratum.

MR. ALLEN: The Boston District is vitally interested in the proper design of filters for Franklin Falls Dam where the foundations contain impervious material. We shall continue our investigations and a series on filter design at Harvard University this summer. I should like to request that this phase be assigned to the Boston District, limited primarily to filter design under the scope as outlined herein.

MR. CUMMINS: Does Mr. Allen intend to include in his investigation the design of a drainage system for an embankment?

MR. SLICHTER: The outline proposes to study this problem only in connection with levees. I recommend that it be extended to include the adaption of filter systems to flood wall installations.

DR. CASAGRANDE: The change in title does not quite agree with the purpose and scope as outlined. We intended to include only the design of filter layers and now it is proposed to broaden purpose and scope and to study flow nets for various types of drainage systems. The design of drainage systems is part of the study of seepage through embankments and foundations and should be included under the corresponding heading. The two problems overlap and we need a cross reference to prevent misunderstanding.

CAPT. STRATTON: We will include this under general design and leave the title as it is.

MR. JERVIS: Another problem in drainage systems is the consideration of gravel drains under a large concrete structure, where seepage is large. We should investigate the carrying capacity of such gravel drains and the installation of drainage pipes. We should design drainage systems rather than filters.

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CAPT. STRATTON: We will proceed with discussion of Item C (4) (a).

C (4). - (a). METHODS AND EFFECTIVENESS OF GROUTING PERVIOUS FOUNDATIONS TO REDUCE SEEPAGE.

Purpose: As stated in the title.

Scope and Procedure: Investigation and study of the application of grouting of pervious foundations to reduce seepage, particularly in foreign countries where much work has been done in this field. Planning of a program to be deferred until the investigation and study are completed.

DR. GILBOY: I want to call your attention to the fact that Item (a) does not include bentonite.

DR. CASAGRANDE: These two items are separated because chemicals have been used extensively in Europe and we wish to benefit from existing knowledge and experience. However, for Bentonite no experience is available and we shall have to carry on our own investigations.

MR. PHILIPPE: Our Division is faced with the problem of producing cut-offs with materials that are fairly cheap. We have conducted some investigations with the Carnegie-Illinois Steel Company and they have developed a grout which forms a protective zone against corrosion of steel piling. It is cheap to put in and allows economical use of sheet piling.

MR. HARTMAN: At Fort Peck we were confronted with the problem of grouting sand and made the discovery of a tentative method of applying grout to sand in which we couldn't pump grout directly. The material was pervious and water passed through it readily. We developed a grouting saw composed of a pipe with horizontal jets spaced at various intervals. We used this grout at moderately high pressures. This caused the material to boil up and as the material boiled using small vertical reciprocating motions we could move the saw right along through the material. The material floated in each case would settle down as the grouting passed by. In this manner we obtained a curtain of grouted material. Other districts should be given an opportunity to develop it further.

COL. FLEMING: This grouting with bentonite and chemicals is a matter of vital concern to the St. Paul District. We have accumulated some data on the subject and have translated some German articles on grouting with chemicals. We therefore volunteer to undertake the collection of data concerning practices and results of chemical grouting abroad.

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C (4). - (b). METHOD AND EFFECTIVENESS OF GROUTING OF PERVIOUS FOUNDATIONS WITH BENTONITE TO REDUCE SEEPAGE.

Purpose: As stated in the title.

Scope: Field test by application of bentonite grouting to levees on pervious foundations.

Procedure: Selection of levees which have given considerable trouble because of foundation seepage. Development of the technique through laboratory investigation. Application of bentonite injections and subsequent examination by means of test pits to ascertain the range and degree of effectiveness of the grouting. Subsequent observations under flood conditions to ascertain the effectiveness of the bentonite grouting as a means of reducing foundation seepage.

MR. BUCHANAN: The use of bentonite as a grout for the impregnation and blanketing of pervious materials has been studied at the U. S. Waterways Experiment Station for some time. The tests completed to date have been performed entirely in the laboratory, and were first concerned with the material and its physical properties. Bentonite was used later in connection with a model simulating a levee 25 feet high, to a scale of 1:6. It has been our experience that the seepage through pervious materials can be stopped through the use of bentonite. However, unless the concentration of the grout is carefully controlled, and the material thoroughly mixed, unsatisfactory results may occur. We have been successful in rendering ordinary mortar sand as impervious as clay at a low cost.

Its use as a blanketing material in models has been successful, with reductions in seepage as great as 90%. These blankets have been placed in different ways; first, by the simple broadcasting of the material over the surface; second, by placing a mixture of dry sand and bentonite blended together; and third, by placing a mixture of bentonite and sand in a moist condition over the surface of the model. A recent subproject for further experimentation with bentonite, has been submitted involving the use of a levee approximately 10 feet high, founded on a pervious sand. Thus, the actual work will be carried from the laboratory into the field and much worthwhile information can be obtained, it is believed. At the moment, it appears that we can successfully impregnate and blanket structures in the field with bentonite.

CAPT. BARNES: How did you form your grout curtain? Did you use the method of grouting soil Mr. Hartman described?

MR. BUCHANAN: The grout curtain was formed by using an ordinary well point with an outer protection screen which was adjusted to prevent the infiltration of materials grouted. The actual grouting was started from the top of the formation and progressed downward. Pneumatic pressure was used to force the bentonite grout into the material. The grout radiated from the well point in a uniform manner when we stripped the model.

MR. PHILIPPE: The Zanesville District conducted a series of similar experiments. We found that in models we could use bentonite successfully. During the investigation of a concrete dam site we tried grouting test pits with bentonite and encountered a good many difficulties with grouting equipment. We also found that by the expedient of pregrouting two filter curtains one on each side of the bentonite curtain the conditions were improved. However, bentonite grout was not as successful as we had hoped.

MAJOR SMITH: The Tennessee Valley Authority made very extensive studies to consolidate materials underlying their dikes and were not successful. Sheet piling was finally adopted.

MR. FAHLQUIST: We are starting a field experiment using bentonite in grouting a circular grout pit. We will gladly furnish results.

MR. BROWN: On the Grand Coulee Dam they used bentonite for grouting a leaking cofferdam. The Bureau of Reclamation has information available. It was published in the Engineering News Record.

MR. BUCHANAN: Two instances have come to our attention where the use of bentonite for sealing test pits has been unsuccessful. However, no attention was given to preliminary tests for the determination of the optimum concentration of the material used. The optimum concentration of grout for a definite material must be determined in the laboratory or experimented with in the field before satisfactory results can be obtained. There are many different kinds of this material on the market, which are obtained from various deposits, some of which are in Alabama and North Mississippi. Some of these materials are unsatisfactory. The material which we have used is from a deposit of pure bentonite, existing in Wyoming. It may be obtained in any form, ranging from dust that would pass a 200 mesh sieve to a size which appears much like sand. It may be purchased in sacks, 100 lbs. size, or in bulk directly from the mine, to be crushed on the job. The use of the granular form was found desirable in that it mixes most readily with water. The grout mixture must be used promptly after being mixed. Efforts to seal one of our test pits were unsuccessful, because the grout was mixed and left to stand for approximately one week before being used. If it is used promptly there apparently occurs a delayed swelling, and material seals itself in place.

COL. FLEMING: The Twin City Sanitary District used bentonite very successfully to seal construction joints in sewers driven through sand stones. The results of their work are available if needed and St. Paul will get them.

DR. RUTLEDGE: In the setting up of bentonite the rate of action can be controlled by the addition of potassium salt, securing any rate you want.

CAPT. STRATTON: We are now ready to discuss specific proposals on Item C (4) (b) on page 5.

MR. SENOUR: I recommend that the present experiments be continued by the agencies now performing them and that their results be made generally available through some central agency.

MR. PHILIPPE: I recommend that the method of grouting developed by Mr. Hartman be further developed, using the grout pipe which is jetted on one surface and forcing the grout through those jets.

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CAPT. STRATTON: We will proceed to Item D, "Stress Distribution and Settlement of Embankments and Foundations" for general discussion.

D. - STRESS DISTRIBUTION AND SETTLEMENT OF EMBANKMENTS AND FOUNDATIONS.

Purpose: The investigation of this (the first phase of the design problem as given under Item A) fundamental design problem provides an approach to an understanding of the forces to which embankments and formations are subject.

Scope: Field observations on existing structures, laboratory observations on models, research on theory of stress distribution.

Note: The stress distribution within earth dams and their foundations is a very complicated problem and among others dependent on the relation between the stress-strain characteristics of the materials in the dam and in the foundation. Depending on this relation, the stress distribution may be widely different in structures which otherwise resemble each other closely.

Procedure: Careful observation of both the horizontal and vertical movements of various points of the base of the dam, its sides and the crest, together with direct pressure measurements by reliable pressure cells. Comparison of observed data with the results of theoretical studies.

MR. PHILIPPE: In studying the movements of Clendening Dam, we were handicapped because there was no method to determine internal stresses. We, therefore, recommend that accurate pressure cells be developed. I have started work along this line and have developed a cell which may or may not be successful. There should be some method of measuring pressures but until we do find methods we must take personal opinions for our stress distributions.

CAPT. STRATTON: The conferees covered this subject pretty generally while at Harvard. Perhaps that discussion obviates the necessity of prolonging it here.

DR. CASAGRANDE: There is another way to approach this problem if pressure cells fail. Assuming that we know the stress-strain relationships of the materials in the embankment and its foundation, we could determine approximately the stress distribution from measurements of the movement of a number of points within the embankment, particularly along the base.

MR. BUCHANAN: Observations like those outlined by Dr. Casagrande were once attempted. Plates were placed in a boring on the centerline of a structure and were imbedded at the top, middle and bottom of the various cohesive strata encountered. Each plate was connected by a heavy wire to a counter-weight located on the top of the structure. It was planned to determine the deformation of the plates by observations of the displacement of the wires at the surface, thus determining the differential settlement within the foundation medium.

MR. HOUGH: I believe that the scope is limited too much. The scope indicates that we have perhaps gone too far with theoretical studies and should now place emphasis on field work. Further development of theoretical study should not be excluded.

CAPT. STRATTON: It is not our intention in preparing this agenda that study be limited to field observations only.

MR. JERVIS: There are many locations where soft, yielding foundations exist and where such verification by field models should be made. The stress distribution by measuring strain would have to be computed.

MR. BLOOR: We hope to make some observations of this sort on the Zanesville dams. We are also engaged in the design of flood walls. We have made photo-elastic tests of foundations for stress-strain characteristics. We intend to work on this problem if we can get the necessary approval.

MR. HARTMAN: This problem should not be assigned to any one district or any one man, at least not until we have a better idea of what we want to find out.

MR. MIDDLEBROOKS: We might get an expression of opinion from Dr. Casagrande and Dr. Gilboy to find out more about this stress distribution and the settlement of foundations and embankments. We can develop more on the apparatus we have now than on that we had in 1931 and 1932. We also need to get that stress distribution from pressure cells. I do not think we should wait to put in the hydrostatic pressure cells until we develop the earth pressure cells. We would like to see a minimum amount of installations in all structures. Whether the new apparatus would give us a complete picture or not I do not know. It would be better to have a partial one than none at all. In other words, if we go on the suggestion that Dr. Casagrande made, we know we can make up some apparatus that would give us the differential distribution. Some other apparatus would give us hydrostatic pressure. We need only know the hydrostatic pressure that exists from flowing water through it, but, as brought up in the discussion the first two days, we need the hydrostatic pressure to get the hydrostatic excess in place in the foundation. I think those problems we can attack right away without waiting for the development of earth pressure cells.

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CAPT. STRATTON: Anything further under Item D? If not, we will proceed to discussion of Item E "Stability of Embankments and Foundations".

E. - STABILITY OF EMBANKMENTS AND FOUNDATIONS.

Purpose: To verify predicted behavior of embankments by theoretical studies and test embankments.

Scope: Field study by means of test embankments of scant dimension and radical design to provide direct evidence of the accuracy of the behavior predicted by the accepted analyses of design.

Procedure: Construction of test embankments of scant dimension and radical design and study of their behavior by observation of settlement, seepage, etc. These embankments should be analyzed for stability by accepted design methods and the test results checked against their predicted behavior.

Note: These same test embankments can be used for observation for settlement, deformation, pressure distribution and seepage.

DR. GILBOY: This proposed scheme of studying the stability of embankments by means of test embankments of radical design has arisen out of certain questions concerning the validity of methods which we already have been using without thinking too closely of their applicability and their relation with what actually happens when an embankment fails. I spoke about that in the Friday discussion at Harvard, pointing out that there has been very little information collected and digested on embankments of non-homogeneous sections particularly those which do not conform to the Swedish Circular Method. The idea of this program of study was to try to get some such information by building embankments we expected to fail and then finding out whether they did fail in accordance with the design. In other words, we would use the best methods of analysis we could develop, including the Swedish Method, and then build embankments of such size, both as to height and slope, that they would fail. We could find out from the action of the embankment whether the design was anywhere near the truth, and we could also determine the nature of the failure. I also mentioned in that discussion that we have to look out for places where it would be convenient and economical to perform such tests. Some dam sites are not adapted and some materials are not adapted to those tests. It is a question of finding a district which is interested in this particular problem and selecting a site or several sites where such tests could be made without too much difficulty. I would like expressions of opinions from the various districts as to their particular ideas on that subject.

MR. SENOUR: Could these investigations be made upon structures which have failed by accident rather than by design?

DR. GILBOY: Yes, assuming a sufficient number of structures have failed or will fail in the coming years to investigate without building any new ones.

MR. TORPEN: An excellent opportunity to do this would be in a cofferdam. Unwater the area and have water on the outside and eventually you are going to flood the area. The upstream face of the earth fill may be built at one or two different slopes, the downstream at a small slope for safety during construction. Then you can readily remove the downstream slope up to a point where failure occurs. Meanwhile, you can watch it and take readings.

MR. CROSBY: I believe it is desirable to emphasize the relation of geology and the necessity of cooperation between the two sciences in the study of yielding foundations. We need accurate detailed knowledge of underground conditions, otherwise the best studies will be misleading. We should particularly look for the irregularities. In beds of soft material there is the possible presence of lenses of clay and the best way to determine their presence or at least be warned of the likelihood of their presence, is by a very thorough, careful geological investigation. When that has been done, the soil mechanics studies can be applied to the interpretation of underground conditions. When the consolidation tests are applied to clay, in making settlement estimates, allowance must be made for drainage effects. Failure to recognize these effects may lead to serious consequences. In places where it is so complicated that you cannot be sure of location of these, the geologist can at least point out their likelihood and what the worst condition is so you can provide for that worst condition even if you cannot get the exact condition.

MR. SLICHTER: At the Marshall Creek Dam it is planned during the foundation excavation to make a further study of the soils which were disturbed at the time of the failure of the dam. Some information on slip planes or patterns of plastic failure may be obtained.

MR. WELLS: I would like to ask Dr. Gilboy if we could not get the same information from model studies.

DR. GILBOY: You can if you make the models large enough. Almost any cohesive material will stand vertically for a height of six feet or more, and this is getting somewhat out of the range of the laboratory.

MR. MIDDLEBROOKS: I think there might be one difficulty. All the material in the zone of failure has been disturbed considerably. If the zone of failure is not well defined, it is very difficult to get true samples. After careful measurement, the zone of failure is usually found below the one shown by analysis at time of failure. This indicates that the material had been considerably disturbed. It would be much better to have a small test section and install different equipment to determine the plane of failure. We could work out a scheme on a small scale model and then conduct a field test. The best location for this test work would be a place where levees are proposed. If you could make a workable levee out of the test section you could incorporate it into the new levee. You could not only test for stability of foundation but also for seepage and effectiveness of drains in that section without putting it into the main levee section where there would be objections to experimenting on the same line. I would like to see that considered if you can find a location.

MR. PHILIPPE: I know of several locations, - and there are numerous others,- where there have already been failures. These are logical places to put in test sections. There has been a failure, we suspect how the failure occurred, let's try to reproduce it.

COL. PARK: I should like to offer all kinds of rivers, canyons, soils of all varieties, impervious and pervious. We shall be glad to help out the Vicksburg laboratory and furnish sites for full-scale dams, earth dams particularly.

MR. MEARA: In the Baltimore District there is no question of suitable foundation conditions or materials for dams. This would be a good site to build a dam where we are entirely concerned with the stability of slopes under the section proposed. I suggest that we build a test section at York, Pennsylvania, where conditions are entirely satisfactory.

LT. DAVIS: We are most willing to cooperate with the Mobile District in every way and take advantage of your offer for field verification.

CAPT. KRAMER: Testing structures to destruction is scientifically the way to get at their inherent characteristics and behaviour, but since such tests involve considerable expense and since one test does not prove anything conclusively, you cannot blindly step out into a large program of big test dams. We have built one at Conchas costing seven or eight thousand dollars, about eighteen to twenty feet high. When you get all through with that sort of test you haven't tested anything homogeneous like a steel beam but a structure with a great many variables. It is difficult to control these variables and find out just what was the cause of failure. I see no justification for spending thousands of dollars in large scale research that may produce practically nothing in worthwhile results.

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CAPT. STRATTON: Before adjourning today's session, I would like to get the reactions of our consultants and guests on the proceedings and problems presented today.

PROF. TAYLOR: Of the research problems which have been mentioned today one of outstanding interest is that of filters. The answer to this problem is the description of materials wherein the size of the void openings are such that washing through of the smallest particles of a given, adjacent, finer layer cannot occur. I have been impressed by the large number of practical soil problems in which the question of filters enters. Projects have been planned for studies on this subject and today's discussion has shown a great deal of interest in such work. Therefore we can hope for good progress in the development of this important subject.

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PROF. RUTLEDGE: It is very desirable to begin a fact-finding survey to determine what has been done in each case, on the outside, and in all the various districts. A very important function of this conference should be to advise the establishment of some means of disseminating the results of investigations in order to avoid useless duplication. However, duplication is sometimes advisable in research, advisable among various laboratories and various operators within the same laboratory. In regard to observations of seepage or detérmining the seepage forces and the quantity of water flowing through dams, I have received a number of comments from the various districts on types of equipment in use and experiments which they conducted. The Waterways Experiment Station has found some types not advisable, while other districts are putting them in and expecting them to work.

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PROF. RUGE: I am here as a lobbyist for the earthquake industry and should like to speak of one or two points. We are, of course, impressed by hearing how many dams are going up all over the United States, both earth and concrete. In another generation it will look as if we have turned into a nation of beavers.

Now we are building dams all over the United States, in sections where there isn't much or any dam history from the standpoint of earthquakes. That is a most important point we might lose sight of. The second point is that you are building dams with a longevity of several generations, and designing for two hundred or three hundred year floods. What are you doing about the earthquake problem? You say the chances are we will never have one. The chances are you will have a two hundred year earthquake just as certainly as a two hundred year flood. I speak not only of the western part of the country, but also the eastern part - you are all interested in the design of dams and important embankments. If you will send to the U. S. Coast and Geodetic Survey and get some of their earthquake data you will find it very enlightening. In 1755 Boston had what now would be considered as a very serious shock. At that time, the city was built on the more solid parts of hills. There were no tall buildings and very little earth construction, but if that happened today it might have serious consequences.

Third, as soil mechanics develops quantitatively the natural tendency is to shave off on design. That is the history of every major development of design. What is going to happen as it becomes more quantitative? We are going to make our earth structures as economical as we can. That should mean that we consider more and more features in design as we go along. As we reach the stage where we are building quantitatively I believe it is definitely dangerous to neglect consideration of earthquakes. You probably can divide the problem into two important phases from the soil standpoint. One is when we have a soil which is not completely stable, a grain structure which might be shaken down by rather quick vibration. That is a phase of the picture we must always watch for. The other is entirely a different phase in which you consider that the gravity begins to swing around, while its length may vary as much as 20 or even 50 percent. It raises a question as to the location of the true failure planes. We want to realize there are other things besides the cohesive and shearing strength of soil entering into the problem of stability.

CAPT. DEAN: The major research program such as we are discussing costs much money and takes much time. My advice is, to begin where the other fellow left off, find out what he did, how he reasoned, what he concluded, and what other people thought about his work and his conclusions. We should make the maximum use of all data that has been gathered together. This frequently requires new observations to fill out missing links and essential data left out of reports. Sometimes these missing links are available at the source. Sometimes they can still be obtained from new observations where conditions have not changed in essential details. Frequently, calibration of old equipment by modern equipment is possible. Don't discard or reject old data taken at great expense and great pains by an intelligent person even though he didn't possess present knowledge and equipment. It is easy to start from scratch and make a good showing if you dismiss all old data. It is hard to take a mass of these old data and bring them to a common properly weighed basis. It requires thought and a keen understanding of the other fellow's problem for such a study. It all takes time and more intelligence and mental fatigue and doesn't make as good a showing, but it will give more real progress at first. We should have a new research program using existing structures where we can as a basis for further experimentation for structures that will be built, or are being built, instead of building large special test structures. This again requires thought to get proper control and not have too many variables. By careful selection of sites and by moving the experiments to the site, instead of creating all the ideal sites artificially, better, quicker and cheaper results may often be obtained. Regardless of the technical merits of such an approach, as a practical point of human affairs a long-time pure research program will never get extensive long-time financial support. You must expect to get it as a by-product of practical research justified by major projects existing now. A research job for a current construction job can afford, if a man will chisel out of that money, some money to further pure research. That is the way money for pure research will come. It has come and will come in this way from Governmental agencies or from some big endowment from a private individual.

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June 21, 1938
Morning Session

CAPT. STRATTON: We will begin the discussion with Item F, Design of Dams and Embankments.

F. DESIGN OF DAMS AND EMBANKMENTS.

General discussion at the conference on the separate phases:

(a). Design of dams and embankments from the standpoint of seepage.

(b). Design of dams and embankments in relation to the stress-strain characteristics of dam and embankment materials.

(c). Design of dams and embankments in relation to the stress-strain characteristics of foundations.

DR. RUGE: I believe it was General Tyler's feeling that this is the proper time to bring the earthquake question to a head before the group. Perhaps I can best begin by saying that on Sunday, I spent all day taking measurements with a microscope and when I got outside on the street I could hardly see the signs on the street cars. There is an object lesson in that. If you people don't watch out you will do so much microscope work on your soil mechanics that you are not going to see the big signs there. The earthquakes are a big sign when you are talking about failure of structures and destruction of human life.

There is coming a time when some of your districts are going to be very seriously shaken. It may be in the East just as well as in the West. Some of us, of course, won't live to see it, but others will. If some of these structures go out, it will be a very embarrassing situation for the Army Engineer Corps. I am most concerned with the fact that we try to hide our ignorance of this problem to a certain extent. In some of your dams you have employed certain rule-of-thumb methods for earthquake design. These rules-of-thumb come from Japanese experience and are based entirely on the destruction of buildings. In soils structures you can't jump from what is observed in the case of buildings into the design of dams or other concrete structures and feel sure that you are on the safe side.

There are several things to be investigated profitably in connection with soil mechanics research. Those of us who are engaged in earthquake research cannot at the same time do soil mechanics research. There are only a few of us in the United States working in this field full time. Naturally, we try to engage the cooperation and advice of every organization in the country. The Army Engineer Corps is most favorably situated to study and look into the effect of earthquakes on soils structures and concrete structures resting on soil. I should like to suggest that some of your districts look into several of the important features of soil mechanics as it pertains to engineering seismology. One of these features is the design of slopes and embankments and also dams against slow variations in gravity, its direction and intensity. The problem is a large one. It involves studying what happens when gravity does not point straight down. I am here trying to counsel not to look into the microscope too much. Let us broaden our knowledge a little bit and admit we don't know what to do about this problem. Let's not shut our eyes to it. The second feature concerns the dynamic strength properties of soils. We talk a lot about stress-strain characteristics and cohesion. What sort of mechanical properties have these soils when the loads are transient? Suppose we are dealing with loads that are on and off quickly. I think it is very important to know something about the cohesive and shearing strengths (the "effective" strengths) under transient loadings, about which there is almost nothing known. Third, what are the elastic properties of the various soils under dynamic loadings? Very incomplete knowledge has been gained on this. Many soils, particularly the cohesionless soils, exhibit very elastic dynamic properties.

The fourth and perhaps most difficult part of the problem is, what water pressures are involved due to earthquakes? There are some theoretical bases, if you want to make enough assumptions, to tell you what these water pressures might be. What the water pressures are in an actual earthquake is unknown. In your designs, to a large extent, you attempt to make them as quantitative as possible, and yet, in a large measure, we deliberately shut our eyes to features which may be of almost equal importance to those we are worrying so much about. I don't want to minimize the importance of soils research studies, but you should maintain the proper perspective and not become too much absorbed with some of the less important details without first weighing the problem as a whole.

MR. PHILIPPE: We have considered the earthquake problem in concrete dams but we have not thought about it seriously in soils structures. We have built photo-elastic models of embankments loaded with water and set them through gyrations which probably have no connection with an original earthquake. Considerable disturbance occurred but we concluded that these disturbances would be taken care of by the factor of safety. Is this a possible line of attack?

DR. RUGE: I doubt it very much. The easiest problem for us to attack is the concrete gravity-type dam. Although a little outside soil mechanics, it is undoubtedly the most amenable to laboratory analysis. We should have the same problem in trying to deal with models of earth structures as you people have, plus the difficulty of making a dynamically similar design for an earth model.

CAPTAIN KRAMER: Despite Dr. Ruge's authoritative statement that earthquakes are likely to occur anyplace in the East as well as the West, I have a reasonable doubt as to what the effect of an earthquake would be on a properly constructed earth structure of cohesive material. Any concrete gravity structure is subject to overturning or sliding failure, from a combination of forces, all of which are susceptible of analysis. Analytically, such a structure, is designed to be safe by simple mechanics. An earth structure, however, by any analysis that can be made as to static stability,- (I am not referring to the cohesiveness or shearing of the material but to sliding or overturning), is so far beyond any reasonable factor of safety structurally, as compared with a building, or a concrete dam, that I don't see where the suspicion of doubt as regards earthquakes enters into the structural stability of a conventional earth dam, unless you can say that the earthquake changes the behavior of the material. If it does, we are just out of luck. An earth section, to repeat, is inherently stable because it is designed against seepage and shear, so that as far as the mechanical application of simple mechanics is concerned, it is safe to say that none of them are subject to overturning or sliding on the base.

MR. JAKOBSEN: We have, in southern California, an earth fill dam which was damaged by earthquake. We have pictures and a description of it. There are several large earth dam projects in our District so the Division office employed Dr. Heiland, Professor of Geophysics at the Colorado School of Mines and President of the Heiland Research Corp., Denver, Colorado, to make an investigation of the earthquake danger to the Hansen Dam. It is strictly a problem of dynamics. I have just examined the report very briefly, but suggest that anyone interested get in touch with the Division Engineer.

MR. CUMMINS: Has Dr. Ruge any information on the history of earthquakes on Japanese dams? There are a good many dams in Japan that are built of cohesionless materials. There should be some information available on the effect of earthquakes on these dams.

DR. RUGE: I do not recall any information on dams in the 1923 earthquake. Mr. Crosby might be able to tell you about it. I might refer anybody who is interested to Freeman's book on earthquakes, a valuable compendium of general information. The main point I meant to bring out yesterday is that we are building earth dams in sections of the country where there is no earthquake history with respect to dams to serve as a guide to judgment.

MR. CROSBY: If anyone is interested, he should get papers from the Santa Clara Water Conservation Committee in San Jose, California. They built earth dams, some of them very large ones, and made provisions for earthquake stresses which occurred right under one of these dams. They made a definite study which would be interesting to anyone studying earthquake stresses in dams.

CAPT. KRAMER: Granting that you are going to allow for earthquake stresses, just how do you modify the design of an earth structure for the earthquake phenomena?

DR. CASAGRANDE: A study of the effect of earthquakes on earth dams requires first of all a thorough knowledge of the soil properties. It is hopeless to attempt to analyze the effect of dynamic forces on a mass of soil if we are not even able to determine with sufficient accuracy the effect of static forces.

When transient loads are placed on soils, we do know that cohesionless and cohesive soils behave differently. A dense cohesionless soil will tend to expand and if the voids are filled with water, additional effective stresses are set up in the dam which increase its shearing resistance. The dam seems to be bracing itself against such forces which tend to destroy it. On the other hand, a loose fine sand will tend to reduce its volume under the effect of a disturbance and since the excess water cannot drain sufficiently fast from the pores, the water will carry a portion of the stresses, thereby reducing the shearing resistance of the mass. A serious slump of the entire mass and, in the worst case, a flow slide may result. I believe that if we place a cohesionless material in an embankment well below the critical porosity, earthquakes cannot destroy such an embankment. As to the distinctly cohesive soils, so far we have no evidence that transient loads can produce failure of an embankment, if it is able to withstand the permanent static forces.

DR. GILBOY: We haven't made any concerted attempt to study this problem but Dr. Casagrande's remarks are very much to the point. We can say this, that the dams that we plan to design and build will be handled according to the best methods available at the time. The best methods of compaction, in the case of cohesionless materials, will result in a dam below the critical density so that earthquakes won't affect it. In the case of hydraulic fills there is a danger that due to earthquakes the transition zone between the core and shell, which consists of fine material rather uniform and fairly well segregated in places, may, although stable under normal circumstances, exert a fluid pressure. That is one reason why a hydraulic fill dam should be designed to resist full fluid pressure from the core. The design method I developed was an approximation in the sense that if the dam was stable with full hydrostatic pressure from the core and the core then consolidated, the design would be on the safe side. With a rapidly consolidating core we are perhaps too much on the safe side. We will have to develop some scheme to determine how hydrostatic pressure could be reduced during construction, knowing the weight of the core material, and its consolidation characteristics. If we could reduce it to 50 or 75% of full hydrostatic pressure the section could be correspondingly steeper. In view of the possibility that the loose cohesionless material around the edge of the core may become almost liquid I don't think we ought to be less conservative in hydraulic fills than to design for full hydrostatic pressure. If we do that, both the rolled fill and hydraulic fill will have sufficient additional factors of safety to take care of earthquake pressures. I would like to know more about the nature of changes of gravity forces in direction and pressure. I think we might well collect information on that point.

MR. SENOUR: Dr. Gilboy has specifically recommended that in the design of hydraulic fill dams we consider the cores to act as a fluid. Is there a consensus of expert opinion on these various points?

MR. JAKOBSEN: Dr. Heiland concluded that no earth dam could be affected by earthquakes unless in resonance with the earthquake vibrations. Tests have been carried out in detail in connection with the Hansen Dam. The safety does not depend upon the base width when the dam has a triangular section, but upon height, density and elasticity. The only thing we can change is the height. Dr. Heiland has gone into the subject very thoroughly. I think this is the first time an earth dam has been treated from a dynamic standpoint.

DR. GILBOY: There seems to be two different aspects to that problem. We can treat the dam as a single mass and study the resonant effects; but in addition, with a loosely deposited fill we must recognize that there is a tendency toward compaction when displacements occur.

MR. JAKOBSEN: But the displacements are very small.

DR. GILBOY: That is all you need. I certainly would be afraid of any displacements whatever, in the case of a loosely-deposited hydraulic fill.

MR. JAKOBSEN: The danger is from resonance occurring very suddenly.

DR. GILBOY: The worst condition where resonance occurs would be in the case of loosely deposited fill where there is enough vibratory force in that material to cause displacement.

MR. JAKOBSEN: Assume a properly constructed dam.

DR. GILBOY: A hydraulic fill dam is properly constructed provided you make enough allowance in the shell to resist full hydraulic pressure.

MR. JAKOBSEN: Is there any record of any hydraulic fill dam having been damaged by earthquakes?

DR. GILBOY: None that I know of. It all comes back to the point that if we are going to make our designs more and more quantitative so that we use less and less material in the section, then we have to watch for the possibility of dynamic forces producing any effects that we have not anticipated. So if we are trying to work out a scheme of hydraulic fill dams with less than full hydrostatic pressure against the shoulder, we are going too far along the line we have been warned against.

MR. JAKOBSEN: Dr. Heiland concluded that the dam was in less danger if the reservoir was full.

DR. CASAGRANDE: I agree with Dr. Gilboy that hydraulic fill dams, as they have been built in the past, should be designed for full liquid pressure of the core. However, it is possible to build hydraulic fill cores in such a manner that full liquid pressure cannot develop. We could compact the fine-grained beach material by means of vibration machines and the core material by means of internal vibrators which are pushed up and down, thus preventing the core material from forming a very loose structure.

MR. SENOUR: If you use full liquid pressure, should you use a factor of safety of one and one-half in addition?

DR. GILBOY: I have investigated several hydraulic fill dams which have a factor of safety ranging from 1.1 to 1.2. In the Cobble Mountain Dam is a section which, from the standpoint of experience and judgment, appears to have a high factor of safety due to tremendous rock fills at the toes. Yet, when figured on the basis of full hydrostatic pressure, the factor is about 1.2. This is about the highest factor I have seen. When you have a factor of safety of 1.2 against the full hydraulic pressure you have taken care of practically everything that can possibly happen. You have made allowance for the possibility of displacements and the tendency to flow-slides due to vibrations.

CAPTAIN STRATTON: I take it as a consensus that hydraulic fill dams should be designed for full liquid pressure in the core, subject to modification by our consultants.

MR. MIDDLEBROOKS: The safety factor depends upon the type of material. You are assuming that all material in the hydraulic fill is above critical density.

DR. GILBOY: In a dam like those in Miami Conservancy District which have in the core a large percentage, perhaps 40%, of clay, the core material even though partially consolidated develops a high shearing strength. I am not worrying about that stuff being broken up by transient loads. Between the center part and the coarser material which also is stable, there is a transition zone which consists of fine sand. That is what I am worrying about.

MR. MIDDLEBROOKS: If it is dense enough it could have no effect upon it.

MR. PHILIPPE: I would like to ask Dr. Gilboy if the Miami dams were included among the dams of which he has computed the factor of safety. Those dams have been subjected to minor earthquakes.

MR. TORPEN: An earth fill dam constructed for absolute safety in regard to stability, seepage, etc., has ample proportions in general. We expect to have earthquakes everywhere but of various intensities. I believe the normal earthquake would not do a great deal of harm to ordinary earth fill dams. A dam with hydraulic core should be designed for full liquid pressure of the core, but to add additional safety factor at this point seems questionable because by the time the dam is constructed the core begins to consolidate and will have enough firmness for earthquake shocks. Before the dam is fully constructed, no great harm can happen because there is no water behind it. The worst thing that could happen would be damage to the fill itself. For additional slope you wouldn't put a 20:1 slope on it and you wouldn't expect one of 10:1 to help it any. It seems questionable whether we will make much change in the shape of dams for earthquakes. We might get an earthquake in some sections where we would have an offset of 6 to 10 feet in terrain. You don't expect to design dams for safety against such a catastrophe. For the normal earthquake or quiver very little need be done to dams as designed now for safety.

MR. HOUGH: Assuming we find out how to make dams stable against earthquakes, does Dr. Ruge advise increasing the free board of such dams especially if they have permanent reservoirs to provide against unusual wave action during an earthquake.

DR. RUGE: Just what is a "normal" earthquake? I could shake the Empire State Building down with an earthquake you would not be aware of on the ground. As to the question of wave action, the type of earthquake to produce disastrous waves depends upon the depth and shape of the backwater and possibly upon the very shape of the dam itself. I should like to see some shaking table work done on wave action because I believe it is important and because we don't know anything about it.

MR. SENOUR: How do you design the slope of a dam which has an impervious core and cohesionless shell? There is some disparagement of the Swedish theory. What is the established practice?

DR. CASAGRANDE: A dam of ideal design will contain as much cohesionless material as possible and as little cohesive material as is absolutely necessary to reduce the seepage to a tolerable amount. When using a plastic clay for the core, even one foot in thickness, built up between forms, would be sufficient. In all cases when the core is relatively narrow and steep, and the supporting structural sections on both sides consist almost entirely of cohesionless soils, the stability of the dam can be analyzed like any embankment consisting of cohesionless soil. If the material is compacted below the critical porosity, any slope flatter than the angle of repose will be safe. Even a slope of one on one and one-half should be entirely safe. In comparison a dam consisting almost entirely of clay might require a slope of one on five, or even flatter, to possess the same degree of safety. In such cases analysis by the Swedish method of cylindrical sliding surfaces should be used, combined with a thorough study of the shearing strength of the soil which will be effective under the worst possible condition.

DR. GILBOY: I might add that that design method assumes that the foundations are strong enough to carry the dam. If we have plastic foundations we may have to spread the loads. It also assumes that we have adequate drainage systems. One of the most important things of all is to be sure that the downstream shell is always well drained and that the upstream shell is drained near the slope so that sudden drawdown will not affect stability, and that it will not wash out due to water seeping through it. With those qualifications provided for, then the design method is perfectly sound. I agree with Dr. Casagrande that lots of our dams could be made to conform to those qualifications, and that many dams have been designed on far too conservative a basis.

MR. HOUGH: I suggest that under this topic of design of dams and embankments, we include consideration of earthquake stresses as discussed by Dr. Ruge. I recommend research on the design of dams and embankments to resist earthquake shock.

MR. PHILIPPE: Dr. Casagrande points out that with very limited cores and large cohesionless shells, we have safe designs provided such dams are constructed correctly. On the other hand, if cores become so large as to be the dam, then the Swedish method applies. Most structures are in between, what do you do about these?

DR. CASAGRANDE: In soil mechanics there are many problems which we cannot solve with our present knowledge. We, therefore, change the assumptions so that a solution becomes possible and still the result will definitely be on the safe side. In the case just mentioned by Mr. Philippe, one may assume that there is somewhat more clay in the dam to make the application of the Swedish method possible. I believe that in most cases a combination of the Swedish method and of Dr. Gilboy's method for analyzing the shearing resistance of the shell of hydraulic fill dams can be worked out.

DR. GILBOY: That was the basis for the investigations in the Binghamton District which have not been completed because we bogged down on some assumptions. We dropped the matter because the assumptions in question made little difference in that particular dam design, and everybody was satisfied. We hope eventually to take these studies up again and carry them to their logical conclusions. If we take any cylindrical surface through the impervious core that mass would be stable under the action of cohesion and friction along that surface, plus the restriction of the shell material, which is an additional force to be introduced.

That additional force is estimated by the same scheme as for hydraulic fill shells. With a relatively small amount of additional study we can develop something a great deal more reliable than anything we have at present.

MR. PHILIPPE: I know many people who effectively use the methods of elasticity and plasticity in design of dams. Another line of attack is the photo-elastic method. I have found from experience that the photo-elastic method gives reasonable results. I should like to ask the consultants what their main objections are to that method.

DR. CASAGRANDE: My principal objection is that in the stability analysis of soils we do not deal with elastic materials. When designing earth dams for safety against failure, we allow stresses which go far beyond what may be called the elastic limit of the soils. Therefore, the photo-elastic method can yield only a very approximate picture of the stresses which will develop within an embankment and its foundation. It is a helpful additional method to determine one of the limits of the range in which the actual conditions will fall, and which should be used to supplement other methods. However, I would never use it as the only approach to the problem.

DR. GILBOY: I have no objection to the photo-elastic method. It is a perfectly good way of finding out something about stress distribution, particularly at the junction of the dam and foundation where the stresses are of a very complicated nature. The only question in my mind is whether the material we use in these studies conforms to the physical properties of the soil we are using in the dam and in the foundation. The photo-elastic method can give us very good indications, but I think it ought to be supplemented and checked by other methods as well.

MR. CUMMINS: At Fort Peck we went into the photo-elastic method of analysis quite extensively. It took us quite a while to develop our technique. In our test models we obtained definite checks on mathematical methods. In practically all of our models, at points where we could determine mathematically the stress values, the values obtained from the models checked very closely, within a few per cent. We made embankment models with granular materials and cohesive material. These were placed on a gelatin foundation and in most cases, yielded very satisfactory results. Along the lower boundary of the foundation layer, at the base of the model, that is, at the so-called rigid boundary where we thought we could obtain mathematically computed stresses that were fairly correct, the photo-elastic stresses along the same location checked very closely in every model.

DR. GILBOY: That still doesn't answer Dr. Casagrande's objection. When you make these computations you make assumptions of elasticity. That is the basis of the computation. You then make a model test on gelatin, a more or less elastic material, and it is not surprising that you get a check, you ought to. It doesn't answer the objection that when the stresses are so high that the elastic theory no longer holds your gelatin models will not give you the correct answer.

MR. MIDDLEBROOKS: How much of an approximation is it to assume that the soils will act elastically? Is that approximation closer than all the approximations we make in the slide analysis? In the future I think we shall be able to further develop it, but at the present time the gelatin model supplemented by best mathematics will give us a closer agreement than the circular slide method.

MR. PHILIPPE: In studying the movements at Clendening Dam, we built gelatin models and treated them photo-elastically. By slowly heating the models we were able to reproduce the failure that had occurred in the dam. Curiously, the movements on the model checked with those which we had previously observed on the dam, even to the effect of consolidation. The stresses determined from the photo-elastic model, compared with the shearing strengths of the soils as determined by the direct shear method, gave the factors of safety indicating failure, and yet you say photo-elastic models are not acceptable. This is evidence to the contrary.

MR. CUMMINS: We analyzed the closure slopes of our dam and the results of our stress analyses checked excellently the results of the model tests. The computations indicated that the factor of safety was ample at a point where a minor slide actually occurred. The photo-elastic model, however, indicated this point. I think all the evidence indicates that the photo-elastic method can be developed until it is fully as reliable as any of the rest.

MR. SENOUR: May I ask if these slides were also investigated by the circular method?

MR. PHILIPPE: Ours was. By the circular method our dam appeared to be safe.

MR. BUCHANAN: We are concerned with structures and foundations. A structure may be designed so as to be perfectly safe but if it is placed on an unstable foundation it will fail. We should consider whether it is a foundation failure or a structural failure. Some might think that a structure when analyzed by one method and found to be safe should not fail, and yet it may fail.

CAPTAIN STRATTON: There is one additional item of research, regarding the design of drainage systems, which was fully discussed yesterday. Investigation would include the study of filters.

MR. HANSEN: The Little Rock District at the present time is conducting tests on filters. The drainage system at two dams consists of perforated pipes 24 to 36 inches in diameter, placed in a filter bed at the down stream toe of the impervious section of the embankment. It is absolutely essential that the filter be designed so that the fines from the core or foundation will not pass through the filters and clog up the pipe. Man-holes have also been put in at intervals so that it will be possible to clean the drain pipes.

MR. PHILIPPE: The Ohio River Division is designing levees which take advantage of drainage. We, therefore, request that these studies be extended to all reasonable degrees.

CAPTAIN STRATTON: I might add that the Boston District is concerned with the design of filters and we propose continuing our studies to encompass the scope as given here.

MR. CROSBY: I have a research problem that might be taken up by some district; namely, the plastic flow of weak rock. It is known that even marble flows or deforms very slowly under very light loads, loads well below the shearing strength. It is, however, so slow that it is not important with rocks as strong as marble. Its importance with very weak rocks, such as soft uncemented shales and unlaminated shales, is unknown. This could be studied both by laboratory and field methods. In the laboratory a specimen of a weak shale should be put under constant moderate load well below the shearing strength and left there for months. You can't accelerate this or you will depreciate the results. Experiments I have made were not conclusive, but indicate that results can be obtained. Field observations on complete structures would take years but might give valuable results. We very much need this information to determine whether deformations in many of the rocks are sufficient to be serious to structures upon them. If you wait until the actual problem arises it would take too long to carry out the work. Therefore it is very desirable that such tests be begun upon suitable materials. In connection with artesian aquifers, it is known that an increase in pressure tends to squeeze water out of this material. This may have considerable to do with the phenomena of sand boils. If you pump out water from an artesian aquifer there is a tendency towards settlement of the surface. The practical importance of this is not known. It is an interesting subject for a research problem. The third point I wish to mention is in connection with the chemical properties of the soil. A general study of the chemical properties is out of place in this program. In the arid regions, however, there is a possibility of having soils of soluble constituents. In some cases it might be well to investigate the chemical stability of soils.

CAPTAIN STRATTON: These items are of very definite importance but we will not discuss them unless someone cares to do so. Captain Kramer has a sketch of a hydrostatic pressure indicator which has been successful at Conchas Dam. This device was developed by the Bureau of Reclamation. If anybody is interested they may talk to Captain Kramer. We will proceed with discussion of "Construction Control".

G. CONSTRUCTION CONTROL.

General discussion at the conference.

COLONEL CUNNINGHAM: I understood this was a research program and I object to the idea that research cannot be applied to the term "construction control". My point of view is somewhat different. We are certainly interested in the fact that design is followed during construction. We have reached the construction stage and we would like every possibility of advice, further experiments, or further observations to insure that construction follows design. It is for that purpose that the point was raised. We have to put your design into the specifications. The contractor has an interest in this. The proper methods of control of construction must allow a contractor reasonable progress, reasonable non-interference with progress, and if we interfere with that we must pay for it. So, starting from design and placing that design into plans and specifications, we would like to receive from the design the necessary control of construction. We should be practical in relation to contract work. It is the job of design, to indicate what that control should be. A simple, homely illustration - I don't think I should have to figure out as District Engineer how many settlement hubs to put in. If there should be a difference in foundation conditions, the design should tell us where, how many, and what observations to make. That appears in the specifications and in relations with the contractor. The best model that we have of our construction is the dam itself. We are building those and are willing to use any sort of practical observations to assure us by long-time observations what is taking place in the dam. If there are any other means besides pressure cells by which to continue to take observations of undisturbed samples from any part of the dam, that would be a proper subject of research. For those reasons, we raised the point of field control.

CAPTAIN KRAMER: I wish to discuss very briefly the procedure followed at Conchas in rolled fill construction in the prewetting of borrow pits. In the middle west and east, the standard procedure is to add water on the fill when additional moisture is required. The Reclamation Bureau and other agencies in the southwest and west follow the opposite procedure of prewetting the material in the borrow pits. In Conchas we have followed the prewetting procedure. It is perfectly logical and simple. It permits the functioning of equipment with much less grief and with much more accurate control of the moisture in the embankment than is possible by direct addition of moisture in the fill. The close moisture control is shown by the following case. We were virtually without water for six weeks during a recent drought. We had irrigated out pits one or two months in advance, giving the moisture an opportunity to percolate through before excavating the material. During that drought period of six weeks, no water whatever was added to the borrow pits. We had enough prewetted pits and the material was moist enough to permit close moisture control of the embankment. We were able to continue operations during a drought period. This procedure of prewetting borrow pits results in the very best construction control and should be followed wherever local conditions permit.

CAPTAIN DEAN: Construction control and research for construction control are most vital to the Department. We are essentially a construction agency. I would like to cite two or three matters of construction control which were highly controversial in the New Orleans District. We were constructing clay levees. The land was soft and was covered with water so it was impossible to use machines which tracked. We used two types of floating equipment, hydraulic and a long-boom clamshell. There are a great many theories about constructing levees of clay. I have found that the only clay to use in hydraulic construction was clay that came out in good-sized balls of undisturbed material. In every case you wash off a lot of stuff in the course of dredging. The contractor naturally always wants to retain fines. According to my theory, you can get satisfactory results only if you make every conceivable effort to drain out all material that will wash off, just as rapidly as possible, from clay balls piled up like coarse gravel. The contractors argue that you should retain fines to help cement the material. This matter requires research. The clamshell was required in some cases to dig from dry pits. The soil was so impervious that it was possible to keep the pits dry, but it is a lot easier from a construction standpoint if no effort was made to keep the pit dry and the clamshell simply took material out of the water. To what extent should we exert construction control and insist on dry pits? It costs a lot of money, and is a question of rotation of pits. To what extent should we permit the contractor to move in with the clamshell toward the toe of the levee and dig shallow pits to extend his reach and place the entire material in the section by clamshell methods with or without refill of the pits? One other point was the placing of material with these clamshells so that the material on the far side, which was beyond the reach of the boom, would get there by building up the near side a foot or even higher than the design section, and then letting the material squeeze down to the far side. No one disputed that it was bad practice, but was it bad practice to the extent that we should pay three or four times as much for other methods? All these methods of construction control are of vital importance and require definite research.

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The foregoing completes the record of the "General Discussion" and "Suggestions for Research" under the general heading of "Specific Program" as outlined in the opening remarks of the chairman. The remainder of the conference period was devoted to the discussion of "Special Problems" suggested by the various District and Division offices represented at the meeting. This discussion is recorded on the following pages.

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CAPTAIN STRATTON: We will begin with the discussion of Item (1) under the section on Special Problems. It is requested that each district state its problems in a more specific manner.

H. SPECIAL PROBLEMS. - To be discussed at the conference.

(Items enumerated, unless otherwise indicated, are those in letter of Boston District of June 3, 1938.)

(1). The practicability of glass flume models in studying seepage through earth dams. Item B (1) (a) - Conchas District.

MR. BERTRAM: We would specifically ask how the seepage lines as obtained from a glass flume model of homogeneous sections - that is, each part of the model being homogeneous - would compare with the seepage lines obtained from the prototype which is to be built by rolled fill methods. The material was placed in the model in layers. Material, of course, was uniform and the sand which was used was uniform.

DR. CASAGRANDE: We can never expect to find a close agreement between the flow net obtained from such a homogeneous model and that obtained from the prototype for the simple reason that the material in the borrow pit is not homogeneous and, therefore, each layer in the dam will be slightly different. Only on the basis of a thorough study of the variations of the material in the borrow pit is it possible to arrive at an estimate of the variations in permeability which we should expect in the dam. From this estimate one can compute the average permeability in horizontal and vertical direction, and then study the flow through a model in which the sides are so steepened that it corresponds to the transformed section.

MR. HOUGH: We all recognize that it is extremely difficult to get exact similarity between a seepage model and the prototype. We should not look to the model for exact results which are directly applicable. The model, however, serves a very useful purpose in studying proposed changes in different features of earth dams. The entire flow net through a dam and embankment is affected by many features such as relation of the downstream shell and core and the size and location of drains and many other features of that nature. Those are the features which are particularly difficult to investigate analytically, and these things are clearly shown in a model even if the model is not exactly similar to the prototype.

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CAPTAIN STRATTON: We will proceed to the discussion of Item (2).

(2). The value of cut-offs or Keywalls through earth dams built on relatively impervious foundations. Item B (1) (b) - Conchas District.

CAPTAIN KRAMER: This is essentially an engineering question and not a question of research. The conventional design of an earth dam usually involves cut-off walls. At Conchas, we are considering eliminating these cut-off walls which complicate the rolling of the fill. Are we just blindly following precedent in putting in cut-off walls that might better be left out of the design?

DR. GILBOY: I agree with Captain Kramer; we stick them in just because someone else has stuck cut-off walls in. These walls don't fool the water in the least. The same thing holds true in the case of cut-off trenches. They are seldom useful in regard to the seepage of water. The only use for a cut-off is in a case where you have reason to believe or know that the land on which you are building might at one time have been drained so that you are not sure whether a drain tile will run upstream or downstream in your dam. The best thing to do is to dig an exploration trench and make sure none of these conditions exist. As far as a cut-off is concerned it doesn't amount to anything.

MR. HOUGH: Do the consultants think there is any value in digging exploration trenches to determine whether there are any local deposits of soft material at the toe of a dam where the stress is probably a good deal higher than at the center line?

DR. GILBOY: I think if there are any doubts on that point a series of test pits would be perfectly permissible.

MR. TORPEN: Cut-off trenches are very useful in certain cases. Where a dam is founded on permeable material and at a certain depth you find impermeable material, it is generally desirable to cut off seepage entirely. Another case would be laminated foundations of impervious and pervious strata. A cut-off trench at a reasonable depth to reach impervious strata would make the dam quite impervious because the water to get past the dam must enter farther upstream, and the path of seepage would be increased so that the seepage would finally become practically negligible. Even if you do not reach impervious strata a cut-off trench might reduce seepage water by a reasonable amount and the flow net will show lesser concentration of flow.

DR. CASAGRANDE: Cut-off walls in pervious material which do not completely cut through the pervious strata are of doubtful value. One can show theoretically that if the cut-off wall cuts off 99% of the thickness of the pervious stratum, the quantity of seepage will still be 80% of what would flow through without the cut-off wall. Therefore, leaking sheet pile walls are also of questionable value as far as reduction in seepage is concerned. However, partial cut-off walls at the downstream toe of the structure are very effective in reducing the upward seepage pressures at the toe, thereby reducing the danger of sand boil formation and piping. The sheet pile wall forces the concentration of flow lines from the surface to the lower edge of the sheet pile wall, and produces an efficient spreading of the flow lines before they emerge on the ground surface. Also the great danger which results from scour at the downstream toe of overflow dams, which is an important contributing cause of piping, is very effectively counteracted by means of a sheet pile wall at the toe. It is true that such a wall increases the hydrostatic uplift pressure on the apron behind the sheet pile wall. However, these pressures can be easily reduced to a tolerable amount by means of filters which are properly drained through the apron or through the sheet pile wall.

MR. FAHLQUIST: Dr. Casagrande, you have been working with these small scale models for developing flow nets. What are the merits of these small flumes as compared with the larger flumes, models of larger scale?

DR. CASAGRANDE: The type of problem determines which method of model testing will give the best results. The electric analogy method gives excellent results for isotropic materials when all boundaries are known. For problems with a free surface one must find the line of seepage by gradual approximation. The knowledge required to find the line of seepage by trial is also sufficient to find the flow net by means of the graphical method. Hydraulic models, consisting of sand built in a flume, should give satisfactory results whenever all boundaries are known; e.g., the flow through the pervious foundation of an impervious dam. The line of seepage through dams can be determined by means of hydraulic models only for homogeneous isotropic sections. Whenever the dam and foundation consist of materials with different permeability, the limitations of the method often make it impossible to arrive at satisfactory results. The coarsest layers are limited to about the size of Ottawa Standard sand, to prevent turbulent flow. This automatically determines the grain size for the less pervious layers in the model dam, for which the height of capillary rise may become many inches, so that a reasonably accurate flow net through such embankment may require the use of models as large as a room, to reduce the distorting effect of capillary rise to a tolerable amount.

MR. SLICHTER: The graphic method of determining flow nets appears rather complicated when cut-offs and drains are introduced. Could some simpler method involving a combination of the electric analogy with the graphic method or small model be developed?

DR. CASAGRANDE: Could you give me an example of a case which was so complicated that the graphical determination of the flow net was not possible?

MR. SLICHTER: I have in mind a cantilever flood wall supported by wood piling on a pervious foundation soil. A steel sheet piling cut-off wall 15 feet in depth is embedded in the base slab on the river side and a filter drain is provided at the landward edge of the base slab.

MR. BURWELL: The prevention of dangerous seepage under earth dams where the core is carried to bedrock is a controversial problem in the Ohio River Division. I would like the opinion of the Board of Consultants as to the best method of effecting a cut-off under such conditions assuming the rock to be fractured and seamy. As a grout curtain is never 100% effective we are concerned with flow along open fractures at the contact of the core with the foundation. Should we provide a keywall, cut a trench in rock and backfill it with impervious material, or construct a pavement of non-erodible material such as concrete on which to place the core? The Baltimore District has a project where the core is to be placed on bedrock and the Board of Consultants recommended a concrete pavement on the abutments and in the cut-off trench, to eliminate the possibility of flow along the seamy rock damaging the core.

DR. GILBOY: In general, I would say that the solution to any of these problems cannot be stated in general terms, each has to be studied on its own merits. The use of a small key is of no particular value. You probably will need, particularly in the case of the kind of rock about which Mr. Burwell speaks, some sort of grouting cap in order to hold back the pressure. It is all a matter of opinion. I don't see the value of carrying the keywall to bedrock, and trying to compact the material around it. The best thing to do would be to leave the cut-off trench open, work on it, and get machinery down in there, to give you a solid, compact mass in the dam with rock and grouting.

MR. BURWELL: What do you think of pouring a covering of asphalt on the rock surface prior to placing the impervious fill, a non-erodible material to form a junction between the earth fill and the shattered rock surface?

MR. CROSBY: All rock surfaces are shattered. There is always the possibility that if we have one channel left open under core material we may have trouble at that particular point. We can never guarantee the effectiveness of grouting 100%. I still don't see what a keywall does for you.

MR. BURWELL: I am looking for something to form a safe contact plane between the earth and the rock, possibly a concrete pavement or some other non-erodible material to prevent erosion of the core material.

DR. CASAGRANDE: Since we are trying to accomplish something by means of cut-off walls and keys which experience shows is not possible, why do we not go to the other extreme and, instead of trying to prevent the flow, control it by means of appropriate drainage filters in such a manner that erosion cannot occur?

CAPTAIN DEAN: I would like to ask, assuming an impervious rock foundation and abutment, the question of getting a key way in that rock, putting an earth fill into that key way, and then building the dam. In other words, having an earth key way. Do you think that has any value?

DR. GILBOY: No, I don't. The action of blasting that rock makes it worse than before.

CAPTAIN DEAN: On a WPA dam I expressed that opinion. The dam already had been designed and was built with a key way. It was expensive, but I believed the key way served no useful purpose.

MR. CROSBY: It is better to leave the rock alone. Blasting it and putting a concrete cut-off in it does more harm than good.

CAPTAIN DEAN: In respect to clay foundation of doubtful supporting value, there is one serious question of artificial drains. They are unnecessary if you have an exploration trench. In general, it is undesirable to have the cut-off trench in the base of the foundation. Leave the foundation alone.

MR. PHILIPPE: This particular question was a sore point in the construction of the Muskingum Dams. I would like to recommend very strongly that these expressions of opinion be formulated in the form of discussions and circularized throughout the Department.

CAPTAIN KRAMER: Mr. Burwell is going from vertical cut-offs to horizontal cut-offs. He is going to seal the top of the rock with a horizontal slab. What about the plane of seepage between that slab and the embankment? Aren't you just transposing the zone of trouble?

MR. BURWELL: We desire to get away from the short path of percolation provided by keywalls. I am bringing this up for discussion now, as this question arises for a number of dams under design and always results in controversy.

DR. CASAGRANDE: I don't believe that small cut-off walls and trenches have much value in earth dams, but they serve as additional protection beneath rigid dams on fine, loose sand foundations. Without such cut-offs cavity formation may work backward from the downstream tow to the upstream side, finally leading to failure by piping, while a cut-off wall may make the formation of a continuous channel impossible, or delay failure for a sufficient length of time to allow making repairs, if by means of piezometer readings the danger is recognized.

MR. SENOUR: Why do we put cut-off walls around culverts?

DR. CASAGRANDE: For the same reason. Whenever a rigid structure is in contact with soil, cut-off walls are helpful in preventing the formation of continuous channels along the smooth surface of the concrete. But even in such cases I would rely more on proper drainage filters to control whatever seepage may develop.

MR. SENCUR: What is the objection to increasing the path of seepage?

DR. CASAGRANDE: In many cases it represents a waste of money, or at least an uneconomical way of obtaining something which we can get better and cheaper by other means.

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CAPTAIN STRATTON: We will now take up Item 3.

(3). Use of pulverized rock and quarry spoil in filters. Item B (3) - Baltimore District.

MR. MEARA: The opinion was expressed in one of our board meetings that, instead of using sand and gravel filters, the rock excavation might yield enough rock graded from fine to coarse to build a filter. If such a filter could be constructed it would effect a saving in the cost of the dam. We would like some further expressions of opinions.

MR. CROSBY: Pulverized rock? I think you mean crushed rock.

MR. MEARA: We didn't intend to set up a plant and crush rock. It was assumed that there would be some fine particles and some pulverized rock obtained from ordinary blasting. Mr. Burwell has been at the site and knows something about the type of rock with which we are dealing.

MR. BURWELL: The rock mentioned is a hard, brittle quartzite. In blasting we expect to get variable sizes of rock spalls and we feel that by grading this quarry material from coarse on the outside to very fine spalls on the inside, we will get a very satisfactory filter.

MR. CROSBY: I would call attention to the quality of the rock. Ordinary gravel already has had a treatment so that all friable rock was eliminated by its production on the beaches, but if you had a soft, friable sand stone I think that its use in filters would be questionable. If you had quartzite or other hard strong rock I don't see any objection to its use. I think that the grading might be satisfactory. If the character of rock is such that water flowing through it is not going to damage it or break it down, then I think your problem is one of proper grading and design of the thickness, etc., of the filter bed, so that it will form an effective filter.

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CAPTAIN STRATTON: We will call on the Mobile District to discuss Item (4).

(4). Lining of a canal to prevent seepage losses. Item B (6) - Mobile District.

MAJOR SHAW: We have two proposed canals, one of which in particular is well above the ground surface. It goes through a clay substance; the clay in itself is almost impervious. There are numerous cracks in the test pits, we could not even pump water into them. Will the blanket of clay laid in dry, lining and canals about 13 miles long, hold water? How much of a blanket would we need? Could it be mixed in the dry and placed, or should it be placed under water? Which would be the best method of placing the blanket?

DR. GILBOY: I would say that the solution of this problem depends on a study of the local conditions. It is hard to give a general answer. It depends on the nature of the clay that you have available for lining.

MR. BUCHANAN: The material for the lining of the canals contains a large percentage of clay, approximately 40%. Permeability tests, using compacted specimens placed at their optimum moisture contents and maximum density, show that it has a permeability of approximately 10^{-8}. The material appeared workable during the performance of the laboratory tests.

DR. GILBOY: It seems that with the obviously good lining material we have available the best thing to do is make a sufficient number of field tests to determine the thickness of layers we should use. Obviously, if the clay is as tight as 10^{-8} that is a pretty tight clay. It is simply a question of getting it in place conveniently and cheaply. Even a layer a foot thick should be plenty, but it may be too thin to handle economically. That is a question to be decided by making field tests.

MAJOR SHAW: We tried a layer a foot thick by hand methods but it didn't work.

DR. GILBOY: What was the reason it didn't work?

MR. WESTON: We are going to make another test. Perhaps some of the water is escaping along the sides of the pit; maybe the instability of the clay on which the material is placed has something to do with it. There may be fractures in the lining. The top and bottom of one section of the lining roughed up and we lost quite a bit of water. The blanket reduced the seepage by only about 50%. We want to make a test which will eliminate the errors in our technique.

MR. GILBOY: Have you used a ball shaped affair where you have no breaks in the lining? I think it can be done all right, particularly when you have such good material available. Have you considered the possibility of using bentonite covering?

MR. WESTON: We intend to do that later. Our procedure is to place the blanket, test it, remove it, place another one and test that; but we haven't got along that far yet. We intend to make a study of bentonite. It may cost us several thousand dollars before we're through. We don't think we are justified in spending that much money. That is the reason we haven't done it.

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CAPTAIN STRATTON: We will next take up Item (6).

(6). Validity and range of applicability of earth pressure formulae of Rankine and Coulomb. Exact method of sheet pile bulkhead design. Item C (1) (b) - Galveston District.

MR. BUCHANAN: This question is in connection with the design of a sheet pile bulkhead developed by the Galveston District. The elevation of the top of the bulkhead or backfill is approximately 4, and that of the bottom of the slope about -15. It is my opinion that the design of the wall involves no special features other than the pressures exerted by the backfill. The check of the design was based on the method outlined by Dr. Terzarghi in his article: "A Fundamental Fallacy in Earth Pressure Computations".

DR. GILBOY: If you have followed that procedure you have been using the most modern information we have on this subject. I think there is nothing better. I will say this - if you can, during construction, introduce into your present system some means of measuring what those earth pressures are you will be doing everybody a service. Has any work of this kind been done?

MR. BUCHANAN: I do not know. The problem was referred to us for review and check of the basic assumptions used in the design. I believe that a satisfactory arrangement for observing the pressures against the proposed bulkhead can be made and that some very valuable information could be obtained. Have you any suggestions regarding a satisfactory method for determining the pressures against such a wall?

DR. GILBOY: It depends on your design. Is your design braced externally or internally? Is it like a cofferdam or a bulkhead wall?

MR. BUCHANAN: It is a bulkhead wall.

DR. GILBOY: In that case you could put strain-gauges on the tie rods and allow means of getting at them from time to time.

MR. MACNISH: In the Upper Mississippi Valley Division we have constructed a number of steel walls which employ a system of wales and steel tie rods to serve as a reaction or support for the upper portion of the wall. Provision has been made for measurement of the stresses in certain of these tie rods. The selected tie rods together with their anchor piles have been made accessible for strain gauge observations by the construction of timber boxes in the back fill. Although measurements have not progressed sufficiently to be conclusive they appear to be in reasonable agreement with calculated results obtained by the methods advocated by the U. S. Steel corporation for steel walls with tie rods.

DR. GILBOY: Are the pressures greater or less than computed?

MR. MACNISH: It appears that the observed earth pressures, as measured by the tie rod stresses, may be less than the calculated pressures.

MR. HARTMAN: The Louisville District is preparing to make tests in the future. We are going to build a rectangular cribbing. The ends will be sheet piling of two different sizes. We will fill it with water and test it to destruction. The only problem we are up against is the development of a pressure cell which will give us the information we want. We already have test pits in that vicinity but want ideas for a pressure cell.

MR. PHILIPPE: Captain Sturgis, in his absence, has asked me to bring up this question regarding the design of flood walls of the cantilever type with key. In Huntington, West Virginia, the depth of key was determined by conventional methods. The design was submitted for approval to the Chief of Engineers, who objected to the depth of key and recommended that it be increased. On that particular design we were able to compromise. In future designs, however, we will be concerned with the same problem, and must determine just what the depth of these keys should be. Captain Sturgis would like to know by what methods we can attack this problem.

DR. GILBOY: It is very difficult to answer this question in general terms. I would have to study your individual problem.

MR. PHILIPPE: We are asking about a general method of attack. Is it going to be permissible for us to use the circular method or should we go to some other form of test analysis, the photoelastic method, or any other method, or resort to field tests?

DR. GILBOY: I haven't given a great deal of attention to that problem. I will have to think it over before making any recommendations.

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CAPTAIN STRATTON: We will continue with the discussion of Item (7).

(7). Standardization in soil studies and design of earth structures. Item C (3) - Baltimore District.

DR. CASAGRANDE: Does this heading refer to classification of soils, or standardization of laboratory tests? Relative to the design of earth structures, does it refer to the standardization of the factor of safety? Personally, I feel that we know far too little even to talk about standardization.

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CAPTAIN STRATTON: We will now go on to Items (8) and (9). Stability of levees constructed of friable limestone, lime marl, lime shell conglomerate with variable mixtures of fine sand, sedge peat ...... Item C (5) - Jacksonville District. Model tests as a basis for stability determination of levees. Item C (5) - Jacksonville District.

MR. SCHRONTZ: We have a material which is a combination of muck and lime marl. It really is not marl but is simply an aggregate of lime and shell with fine lime ooze or mud. The muck varies from four to fifteen feet in depth. The plan of the levees was to excavate a trench at the toes and fill it with the lime marl material and build up the levee on top of the muck with the muck, rock and marl from the borrow pits. It was thought that the confining of this muck base by the trench fill at the toes of the slopes would be sufficient to obtain stability. In building the levees there was a great deal of mixture of the muck and marl topped with a rock and marl cap varying from four to six feet in thickness. During the last six or eight months the stability of portions of the levee has been questioned due to development of longitudinal cracks along the crown. In some instances these cracks extend down very deeply and, in other cases, they extend just through the crest. Due to the variation of the aggregate in the levee it seems impossible to depend on shear tests to determine stability. The question arose whether it would be more advisable to use some kind of model test.

MR. CROSBY: I take it that the friable limestone and shell conglomerate are pretty well mixed up.

MR. SCHRONTZ: For some distance up from the levee it is 80 or 90% muck, and for the remainder limestone and marl.

MR. CROSBY: There may be some tendency for a limestone and shell conglomerate to break down into smaller pieces.

MR. PAIGE: It is evident that foundation difficulties in this levee are quite possible, and the longitudinal crack is probably caused by flow of plastic material below the embankment. I think the only remedy will be to lower the slope thus spreading the weight over the foundation.

PROF. TAYLOR: During recent years much progress has been made in the methods of model testing, and in knowledge of the theory of models, work in the field of hydraulics being an outstanding illustration. It is recognized in hydraulic model work and is equally true in other fields that it is important to differentiate carefully between qualitative and quantitative results. The greatest problem in all such work is the obtaining of true similitude with respect to the factors being studied.

In the model we are discussing, the strength is the basic consideration and thus the problem is to obtain similitude between the shearing strength of the soils used in the model and those in the actual dam. We know shearing strength is dependent on at least two factors, one being the normal pressure, the other, or others, being independent of the pressure. Thus for any one soil there are at least two factors which must be reproduced in correct relationship. This is not a simple problem. Where more than one type of soil occurs, the difficulty mounts. However, even without an exact reproduction of conditions, valuable results may often be obtained which are of qualitative nature, but they must be definitely recognized as such.

If a model is built to represent a thin slice of a dam, side friction will enter and greatly distort the situation. For example, in Dr. Gilboy's method of analysis of the stability of a hydraulic fill dam, failure along a plane is assumed. To justify this assumption, we made models which could not be depended upon for much in the way of quantitative results but which were of value in a qualitative sense since they indicated that the assumption of failure on a plane was reasonably correct. On the other hand, Dr. Terzaghi's large Retaining Wall model may be pointed out as giving valuable quantitative results, both with respect to the strength characteristics of the backfill and the amount of yield required to produce active pressure on the wall.

Many of us question the dependability of photo-elastic models of earth dams. We feel that for quantitative results they may be misleading. However, for qualitative information, such as for indicating locations where plastic conditions are likely to begin, the photo-elastic method is of considerable value.

Because of the many factors entering into even the simplest of earth dams, it would be very difficult, if not impossible, to obtain quantitative results.

MR. SCHRONTZ: The slope of the levee is four on one on the land side and six on one on lake side. The slopes are very flat.

MR. PAIGE: It still seems to me a case of plastic flow in the foundation.

MR. BUCHANAN: I have observed some of the samples of the material used in the Lake Okeechobee Levees. The muck we have been speaking of is more of the nature of a peat, than the usual silty material frequently called "muck" by engineers. It had an exceedingly high moisture and organic content. It will burn readily, much like cotton in a compressed state. The marl spoken of is an immature limestone. I believe that field models would be excellent. Since little is known of the materials available for the construction of this levee, and since they form such a conglomerate, laboratory tests for design purposes might be most difficult. The sizeable field model is a great help in such cases.

CAPT. KRAMER: In a small test dam it would appear difficult to simulate conditions of hydraulic construction. The manner in which a structure is built is just as pertinent as the material of which it is constructed.

CAPT. DEAN: Where you have a long stretch of dike building and no precedent to go on it is wise to build short sections and get all the information you can out of an actual full-size section. I agree with Capt. Kramer that the method of construction with this type of material is an important factor. Peat suggests the possibility of a high water content. In the Mississippi Valley, where we had a high organic content in the material, the amount of compression that took place was almost comparable with that of a sponge. It was good solid stuff and when it had a chance to dry out it compressed down to a mere fraction of its original size. The earth over it arched in many cases and acres would drop down all at once in one sudden drop of 30 or 40 feet.

MR. SCHRONTZ: The Okeechobee muck contains 60 to 90 per cent organic matter. The dry muck weighs about 37 pounds per cubic foot and the water content is very high.

DR. CASAGRANDE: All that I can say is that I admire the courage of engineers who construct levees of such materials.

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CAPT. STRATTON: We will now discuss Item 10. Investigation of the stability of existing earth structures. Item C (6) - Conchas District.

CAPT. KRAMER: In formulating this question we had in mind a little dam we investigated which was being enlarged for irrigation purposes. I also had in mind Marshall Creek Dam. I would like to get general remarks from the consultants on what they would do if they walked up to a dam of unknown construction and the procedure they would follow to know if it would stand up.

PROF. TAYLOR: The stability analysis of an existing dam and that of one you plan to build are no different in principle. In either case, the analysis is about as good as the reliability of the data you have. The dam which is already constructed presents one advantage in that it may be pictured as a full scale model which has been tested under natural conditions and has either behaved in a satisfactory way or its points of weakness have been indicated. Borings and test pits must usually be used to give the information needed for the analysis of an existing structure. For dependable results, the samples must be as nearly undisturbed as possible.

CAPT. KRAMER: We have been using different samplers to obtain undisturbed samples with a remarkable lack of success.

PROF. TAYLOR: Many improvements in sampling and sampling methods have been made recently and are still being made.

CAPT. KRAMER: We have the latest equipment from Vicksburg.

DR. GILBOY: It takes a certain amount of experience to operate the equipment to get undisturbed samples. You can't always rely on a drilling concern. It may have a fine reputation; it may have had years of work in wash borings, but you can't expect them to go ahead and get undisturbed samples.

MR. HANSEN: We have had some success with sampling tools where the material was entirely plastic. There are certain places where the material was not as plastic as some of the flume clay models. Most of the samples were obtained by test pits. In some cases in impervious material we struck pieces of shale and rock, and couldn't sink our sampling tools. It wasn't entirely the fault of the sampling tubes.

CAPT. KRAMER: Another difficulty we have experienced at Marshall Creek is in obtaining undisturbed samples with the latest methods.

MR. SLICHTER: The sampler we used in the investigation of the Marshall Creek Dam failure was secured from Fort Peck. It had been used with success on the Fort Peck Dam. At Marshall Creek, satisfactory undisturbed samples were obtained only of the plastic clay soils, but samples of soils containing only a small percentage of silt or sand were remoulded by the sampler. Lack of time prevented the development of satisfactory sampling equipment. We now plan to use a sampler designed like the one described by H. A. Mohr in a recent Harvard publication, which is to be driven into the soil by a hydraulic ram. By reducing the interior resistance of the sample tube to penetration and eliminating the jar from driving we hope to obtain satisfactory undisturbed samples.

MR. BUCHANAN: The undisturbed sampler known as the "Vicksburg" or the "M.I.T." Sampler was not designed to obtain samples of embankments containing hard rocks. However, this equipment works satisfactorily in any sort of material ranging from soft, consolidated silts or clays to the hard, well-consolidated clays.

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CAPT. STRATTON: We will now discuss Item (11). Earth dam 100 feet high on loess foundations. Item C (7) - Bonneville District.

DR. CASAGRANDE: We should not treat Loess differently than other soils. In the undisturbed state we know in general its permeability and compressibility. In the remolded state it should be tested like any other more or less cohesive soil.

A considerable amount of confusion has resulted from the fact that many soils which may have been transported by air, are classified by geologists as Loess soils but they were deposited in water and therefore possess properties of ordinary sedimentary soils, and none of the physical characteristics of a true Loess.

MR. CROSBY: The important thing is to recognize loess definitely and be sure it is a true loess and not some similar material that is misnamed loess. The term is used very loosely. A true loess has certain very definite characteristics.

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CAPT. STRATTON: Let us proceed to Item (12). Consolidation characteristics of hydraulic fill dam cores. Item C (9) - Fort Peck District.

MR. CUMMINS: According to Dr. Gilboy's method of analysis, the shell of a hydraulic fill structure might rupture due to the pressure of the semi-liquid core material. In the core of the dam at Fort Peck, we anticipate a large volume of consolidation after the structure is completed. We want to know how this core will consolidate and how it will affect the gross grades of the completed fill. How should we distribute the allowance for the volumetric shrinkage of the core over the completed section? This brings up an interesting point. A stability analysis indicates that possibly there will not be any tendency for the lower portion of the shell to move in toward the core. The core pressure may be always greater than the <u>active</u> pressure that the shell material will exert on the core, and so the line of contact between the shell and the core may not move a great deal as the core consolidates. As consolidation takes place in the outer portion, material will move downward and outward from the upper part of the core. It makes quite a difference in the grades if the effects of consolidation act in this manner. Will the consultants express their opinion on this point.

DR. GILBOY: It is a pretty difficult problem and I don't want to make any definite remarks without a chance to study it in a good deal more detail. I want to point out a minor fact, that the core force is always positive outward, but we still can have inward movement of this surface. The balancing forces will still be consistent with equilibrium as a whole. The development of shearing stress in an outward direction along any plane is a function of the amount of shearing strain. If the core moves inward the whole shell can move in without creating a great shearing strain. This would not affect the stress distribution very much.

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CAPT. STRATTON: We will omit Item (13) here and discuss it later. Let us proceed to Items (14) and (15). Yielding foundations. Item E (1) - Vicksburg District. Plastic flow of clay. Item E (2) - Memphis District.

MR. WELLS: I would like to make the specific recommendation that the Waterways Experiment Station study the plastic flow of clays in connection with their proposed study of the stress-strain characteristics of cohesive soils.

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CAPT. STRATTON: We will now take up Item (16). Limitation of weight of equipment used on the construction of impervious embankments. Item F (1) - Conchas District.

CAPT. KRAMER: There has been a tremendous growth in the size of equipment for rolled earth fill construction. At Conchas Dam the Euclid track trucks have created many construction control problems. The continual passage of trucks instead of producing more and more compaction tends to produce within the limits of the track a definite separation or splitting. These trucks weigh about 30 tons when loaded. Ordinary dump trucks weigh 18 tons. These conditions obtain eight or ten inches below the surface. Instead of a uniform compaction, there is a tendency to break bond. On a dam in Southern California the specifications limited the size and weight of hauling equipment. We have come to the point where the size and weight of equipment is beginning to affect the homogeneity of compacted fills composed of rolled cohesive material. Whether vibration compaction with weight is doing more harm than good is a very debatable question involving construction control.

DR. GILBOY: I agree that this involves construction control and as such is a matter for field observation and a matter of practical limits which can only be determined by finding out how trucks act on the actual job.

CAPT. STRATTON: Is this continued tracking the normal process of placing and loading?

CAPT. KRAMER: This is the normal process of placing. We don't go over the same tracks too many times, but with about four passages you get this effect. We try not to work too constantly in one place, but are bound to get three or four trips. This is more than just a construction problem, for the development of the construction industry is definitely related to the theory of engineering design. We shouldn't design and specify requirements which cannot be built intelligently and economically.

MR. SENOUR: How was this discovered? What evidence had you of the separation?

CAPT. KRAMER: We dug down and put markers in places where we had definite suspicions. We made a systematic study in places where these trucks had made two, four and six trips. We dug out little trenches in these places that we had marked with paper markers and made our observations.

MR. PHILIPPE: During the construction of the Zanesville Dams we observed the same effect and were concerned but did nothing about it. Since that time we have had occasion to open up some of these embankments and if those effects were lasting we couldn't find them.

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CAPT. STRATTON: We will proceed with the discussion of Item (18). Use of culm piles as an integral part of a levee system. Item G (1) - Baltimore District.

MR. MEARA: Has anyone had experience in the use of culm piles as part of levees. The proposal that we give them as little treatment as practicable would save much money.

DR. GILBOY: I have had a good many opportunities to observe that kind of thing in the Wilkes-Barre region. In connection with their use as levees, I think you may be able to get some good information from residents in that neighborhood. Some of them actually did have quite a bit of water on them when flood waters hit there. Coal companies and residents may have actual data available to give you.

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CAPT. STRATTON: We will go on with Item (19). Study of Soils Classification.

DR. CASAGRANDE: It has often been suggested that a standardized soil classification is urgently needed. Numerous classifications have been proposed from time to time but none has proven successful. Most of these are based on grain size distribution. Almost everyone working in this field has passed through a period in which he thought that it must be possible to establish a soil classification based chiefly on grain size distribution. However, as his experience grows, as he encounters incessantly new types of soils which differ in their properties from all others he has tested before, he begins to realize that nature has not standardized the soils and that he will never exhaust the possible combinations of physical properties with which he will have to deal. He will find that the grain size distribution has no simple relationship with the most important physical properties of the soils, excepting perhaps for clean, cohesionless soils. Gradually he will recognize that we cannot possibly hope to establish one classification which will cover all the problems in which we are interested, and that least of all the grain size distribution can be considered as a basis for such a classification.

The best we can do is to establish for every type of problem a tentative soil classification based on those properties which have a bearing on that specific problem. These tentative classifications can be recommended as a guide which can be adapted to specific problems. Certainly, it is premature even to talk about a standardized classification.

Considering the importance and complexity of this question, I should like to recommend that it be carefully studied, perhaps by a special committee of the Engineer Department.

<u>MR. HOUGH</u>: We are all very well aware of the difficulties encountered when we try to classify soil. It is a very difficult problem and appears to need clarification. I believe there is a danger in an attempt to standardize soil classification. The danger is that we take some such system as proposed by the Providence District and sell it to the entire group of engineers using soil and they may think it simplified the problems which Dr. Casagrande has pointed out are very complex. If we have a material which is silt and call it No. 259 and every one knows what that means, engineers might say that you could build dams of it with a slope of 1 on $54\frac{1}{2}$. We should leave soil classification more or less as it is, but should stress the importance of having, besides classification of soil, all the other data which goes along with it rather than try to classify it by information given in the designation of the soil.

<u>MR. FAHLQUIST</u>: The memorandum regarding soil classification was intended to elicit constructive criticism. This classification is not the answer to the entire classification problem but it has been helpful in our work. We would like this body to consider improvement on that classification. This classification is based upon grain size distribution, but our investigations do not end at this point. Numerous tests are made on undisturbed samples covering permeability, void ratio, consolidation, compaction, shear, etc. We carry out the same comprehensive program that Dr. Casagrande has outlined. This classification and method has been used successfully in the Providence District for the last two years. Our investigations have been concentrated wholly in the Connecticut Valley, a glaciated region, but the classification should apply in other glaciated sections of the country.

<u>MR. CROSBY</u>: It might be helpful. There are however some soils with which I have had to deal that would not fit in there at all. I think it is impossible to make any general classification which can be used everywhere. I doubt very much the statement that this would be helpful in geological studies; it seems to me it would be the contrary unless it was accompanied by a thorough description of the soil. If I had just the number to go by, it

would be a handicap rather than a help. I doubt the possibility of a broad application to be used everywhere.

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COLONEL LYMAN: I have always believed in general conferences where representatives of various districts and divisions can get together. I think that it emphasizes and it brings us closer to the realization that we are members not of the Boston District or of the Baltimore District but that we are members of the Engineer Department. I have found that there are some of the assistants of the Department, some of the various officers of the Department, who become so much interested in their own local problems that they forget all about the other districts. I have heard with great gratification the complimentary remarks about the interest in the meeting and that it has served as a send-off, at least as a starter, a stimulant in carrying out this important work. I believe that the success of the meeting is due to your helpful cooperation and I hope that these meetings may go on and that in the not too distant future, we may all get together again. I thank you for coming and hope that you have got some useful information by the exchange of ideas and by what you have seen here as a result of our experiments.

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CAPT. STRATTON: We will omit items 5, 13, and 17 from the discussion entirely.

(5). Development of practical methods for the determination of flow nets in earthen structures. Item B (7) - Missouri River Division.

(13). General Discussion of laboratory testing methods. - See Item D, letter of Boston District dated June 3, 1938. The items which are listed under this heading are too numerous to repeat in this letter. District and Division representatives will be given opportunity to present their problems (listed) for such discussion as time permits.

(17). Demonstrate how a properly designed structure may fail if improperly constructed. Item F (2) - North Atlantic Division.

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We will proceed with the discussion of Item 20.

(20). The proper analysis and methods of deposition for securing large areas of stable fill such as for hydraulic fill airports when subsurface conditions are of the worst (Soft silt and muck). Letter June 6, 1938 - Washington District.

MR. BIRD: It is proposed to build an airport of several hundred acres within the limits of a river. The bottom of the existing stream is about 4 feet below mean low water. The proposed fill will be 12 to 14 feet above mean low water. The foundation for 20 feet will be of the worst type we can imagine, soft silt and muck. Have other districts had similar problems?

DR. CASAGRANDE: To create a stable fill on top of 20 to 30 feet of muck or peat is a very difficult problem. If only little settlement can be allowed after completion of the project, it may be necessary to preconsolidate the underlying strata by making the fill a few feet higher and removing the excess after a sufficient degree of consolidation is reached. Of course, the problem must be carefully studied, particularly as to the procedure of loading, to prevent the formation of slides.

MR. MIDDLEBROOKS: The W.P.A. is working on the same problem in New York City. They are building an airport over muck. Mr. Hough is somewhat familiar with that.

MR. HOUGH: In New York City we conducted some foundation explorations and did some testing in connection with the North Beach Airport. The conditions are somewhat similar to those in Washington, except the fill material to be used in New York is from a garbage and rubbish dump in the harbor. It appears to be a good type of material because it is very light, and one of their problems is the stability of the outer edge of the fill which parallels navigable channels. The outer edge of the fill if at too steep a slope might cause formation of a mud wave and shoaling of the channel. This light fill doesn't do the thing that Dr. Casagrande has in mind, preconsolidate the underground muck. For the Washington Airport I would suggest that consideration be given to a system of drainage pipes placed before deposition of the fill. One delegate to the conference two years ago described such a system and I can get the information used. The drainage system connected with a trench built around the fill. The pumping system was installed to hasten the consolidation to some extent. The question of consolidation characteristics of your fill material comes in. A true silt rather than clay may be sufficiently permeable so that your fill will consolidate within a reasonably short time. With real clay you can't hasten the consolidation of the material even if you overload it. Before you proceed further I strongly recommend a complete series of tests to determine consolidation characteristics.

MR. BUCHANAN: The construction of the island in San Francisco Bay for the Exposition in 1939, presented a similar problem. The island is approximately 400 acres in area, and required the placement of approximately 19,000,000 cubic yards of material. It was constructed in a shoal area ranging from 6 to 40 feet in depth, and on a bottom formed principally of silt. The silt was in a very loose and unconsolidated state, and had never been subjected to any external pressure. Limited preliminary investigation was made of the site prior to the undertaking of construction.

Some difficulty was experienced during the latter part of the construction work which necessitated thorough investigation of both the foundation and the structural material. This was sampled without difficulty. The final construction work was executed in accordance with the recommendations based on the strength of the material. Final results were satisfactory.

MR. PHILIPPE: There is one possible danger in Dr. Casagrande's recommendation, which has once actually happened. In building a broad levee on twenty feet of peat we tried the system he proposes and we got differential settlements between loaded areas so that some places would settle while others didn't. The results were disastrous.

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CAPT. STRATTON: Item 21 has been answered, so we will proceed with Item 22.

(21). The equipment and methods of sampling deep muck and silt deposits. Letter June 6, 1938 - Washington District.

(22). Permeability tests and requirement for levees and foundations when subject to hydrostatic pressure for relatively short periods of time. Letter June 6, 1938 - Washington District.

MR. BIRD: The conditions are very different from those in the lower Mississippi Valley. The stream along which levees are proposed is flashy and water will be against the levee for short periods of time, 24 to 36 hours. What short cuts and what permissible economies can be tried?

MR. BUCHANAN: What kind of material is to be used for the levee?

MR. BIRD: We have an ample supply of almost any material, clay suitable for clay core and silt, sand and gravel fill for either side of the core.

MR. MIDDLEBROOKS: What foundation material?

MR. BIRD: The foundation will be pervious. The nominal water table is 5 to 7 feet below the surface of the ground.

DR. GILBOY: As far as skimping is concerned, the essential element in the problem is to make the impervious section of the levee so thick so that in that short time the impervious section won't become saturated. The remainder of the problem is to drain the pervious foundation by a suitable system of drains so as not to get sand boils at the toe.

MR. PHILIPPE: I have under observation two cases which are diametrically opposite, leading to the conclusion that the rapidity with which these flow systems establish themselves is very largely a matter of construction. We built a small experimental dike some 7 or 8 feet high on a rock foundation. It was rolled very carefully and placed at approximately the optimum moisture. Under those conditions, the seepage penetrated and a flow system established itself in short periods of time, two or three hours. On the other hand, a levee constructed by dumped fill with plenty of air trapped in it has taken a great deal of time to get any trace of flow through the levee. At Lawrenceburg, Indiana, the Ohio River Division will make observations on those effects.

MR. MIDDLEBROOKS: When you have water against the levees for short periods, the foundation is much more important than the levee itself. As soon as water rises up, you have seepage in a few hours or less. A simple drain to relieve the pressure is needed.

MR. HOUGH: There is also the question of organization. If you are working in a levee district where work goes on over a long period of time and you are well organized and have people who, at times of high water, are at their stations and jobs assigned to them, you can skimp more than in sections where the Department goes in and builds a channel improvement and levee and then goes out and has no further concern with the levee. That fact should be considered.

CAPT. STRATTON: The North Pacific Division is designing an earth and rock fill flood control dam in a box canyon with overhanging walls. Information is desired concerning tests to determine the amount of lateral movement that may be expected to occur when settlement and consolidation of the rolled fill takes place. There is a possibility that settlement of the fill may leave voids under the overhangs. The earth section of the dam will be rolled fill of cohesive material.

DR. GILBOY: I will have to look into this problem thoroughly. It might be desirable to blast the overhang.

MR. BROWN: We have considered blasting the overhang but this canyon is 250 feet deep. It would be more economical if we could design a fill which would move laterally and fill any void left under the overhang by vertical settlement.

DR. CASAGRANDE: The use of clay in the dam might prevent the formation of voids through plastic flow. This problem might perhaps be investigated by means of the tri-axial compression machine.

CAPT. STRATTON: The Little Rock District has submitted the following item: Tests for soluble matter in the soil and the correlation of laboratory tests to time and rate in nature.

MR. HANSEN: At one of the dam sites in the Little Rock District, beds of gypsum and materials interspersed with gypsum and calcite crystals have been found in the foundation and abutments. The gypsum is comparatively soluble. We have tried various tests and methods to determine the solubility of this material and have found that the solubility ranges from about 6% to 27%. Since most districts exclude all materials containing over 2% soluble matter, we would like to know if any other districts have had similar experiences with gypsum, and what their tests and methods of testing were to determine the solubility of gypsum, and also if any tests have been made to correlate the rate of solubility in the laboratory tests to rate of solubility in nature. The tests in Little Rock District were made with distilled water, water from the river, the seepage water at the site, and water containing a weak acid. The river water contains approximately one-thousand parts per million solubles, whereas the seepage water contains approximately eight-thousand parts per million solubles. The seepage water is practically at saturation point, providing that there is gypsum going into solution at all times.

MR. CROSBY: I recommended a similar study on soluble soils. I don't know as I can give you specific information now.

MR. SCHRONTZ: The Reclamation Bureau had experience building reservoirs on a bed of gypsum in New Mexico several years ago. The project was abandoned because of the gypsum content. You probably could get some information from the Reclamation Bureau.

CAPT. BARNES: The North Pacific Division has in an immediate program three earth dams for which the prospective material is of a high natural moisture content. An illustrative mechanical analysis of a typical sample of this material runs about 25% clay (less than 5 microns), 22% silt, 46% sand, 7% gravel. The natural moisture content of this sample is 42%, while laboratory tests indicated the moisture content for maximum compaction to be about 33%. These materials are suitable for a rolled earth fill at the moisture content for optimum compaction. A construction problem exists, however, as to how these materials can be dried out to the lower moisture content. Have similar conditions existed elsewhere, and what construction methods have been found to be practical?

CAPT. DEAN: We put similar material in a spoil bank and let the bank rest for a year or more before we handled it from that spoil bank to the final structure.

CAPT. KRAMER: Can you operate over a large enough area so that you could spread and scarify your material and depend upon its drying during operations? By having enough working area and drying area on the embankment you might be able to keep going.

CAPT. BARNES: Unfortunately, these samples were taken in the dry season, the other nine months they are going to be wetter than already indicated.

MR. HOUGH: I would like to ask whether the high moisture content is undesirable because of the optimum or because it is in a fluid state.

CAPT. BARNES: Because it is in a fluid state. You can't move equipment on it. I thought other districts might have faced this same problem and worked out a solution. It appears not, so we will have to work it out ourselves.

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PROF. TAYLOR: The discussion in this conference of the many research and practical soil problems has been most interesting and instructive, and I have greatly appreciated the privilege of being present. Cooperation between workers in practical research work, those in more fundamental research and also the practical engineer who is interested in the results of research, is the one route to real progress in our understanding of the complicated behavior of soils. It is very gratifying to find such a fine attitude of cooperation in this conference.

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DR. RUTLEDGE: I think the discussions at this conference have compared very favorably with the discussions presented at the International Conference two years ago. It has been extremely instructive and interesting. I have been particularly impressed, since the subject of cost has been brought up, by the large amount of observational data now available. A single set of data means very little, but if all this data could be brought together, compiled and evaluated, the Engineer Department would have a very sound basis on which to proceed. To benefit us who are outside the Department, it would be useful if this information could all be compiled in a very brief form, circularized throughout the Department, and made available to engineers who are not in the Department.

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DR. HVORSLEV: I appreciate the invitation of the Department to attend these meetings, and I have listened with great interest to the practical data they have brought forth and to the suggestions of research on various practical problems. The practicing engineer, of course, will be most interested in the furtherance of research concerning the special problems of the projects he is connected with. Such research should be carried out whenever there is an opportunity to do so. It is essential, however, to give some thought to the more abstract problems of the basic properties of soils, such as stress and strain relationships, hydrostatic pressure in the porewater, conditions of failure, etc. Unless our knowledge of these properties is considerably advanced, we cannot hope to get full value of research on more practical problems, nor can we interpret properly the observations made on actual structures.

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CAPT. DEAN: I concur heartily with the remarks about the need and methods of limiting the program to something reasonable. This conference has done much to awaken all of us to the need for specific data. We should get research data whenever that can readily be done in connection with normal construction and then should distribute results to others interested.

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MR. CROSBY: I wish to emphasize further what I said yesterday about the desirability of cooperation between the fields of geology and soils mechanics and to hope that it may become more real in the future. I think it has been recognized by some who haven't put it in practice.

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DR. GILBOY: I have said my say. It has been a great pleasure to see you all and I hope you will repeat this sort of get-together quite often.

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DR. CASAGRANDE: I believe that this meeting has been very successful. Somehow I feel that we could squeeze more thoughts from the brains of some of our members. A number have said nothing but have thought a lot. Perhaps they didn't dare to say it. I recommend that everyone be invited to contribute any additional information by means of written discussions.

In closing, I believe that we should put into the permanent record of this conference something we all know but which has not been expressed in words. Namely, that the success of this conference is primarily due to the work which Captain Stratton has done in preparation for this conference and to the admirable way in which he has handled the meetings. I propose a rising vote of thanks to Captain Stratton.

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CAPT. STRATTON: We may now consider ourselves in adjournment.

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ENGINEER DEPARTMENT DELEGATES TO SOILS AND FOUNDATION CONFERENCE

JUNE 17-21, 1938, AT BOSTON, MASS.

Name	Title	Office
	Office, Chief of Engineers	
F. K. Newcomer	Lt. Col., C. E.	Chief, Finance Division
T. A. Middlebrooks	Senior Engineer	Soils Investigations
	North Atlantic Division	
C. H. Cunningham	Lt. Col., C. E.	Division Office
H. A. Kemp	Principal Engineer	Division Office
S. Paige	Sr. Geologist	Division Office
R. W. Stuck	Sr. Engineer	Division Office
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F. S. Brown	Engineer	Boston District
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D. F. Horton	Assoc. Engineer	Boston District
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T. S. Burns	Principal Engineer	Providence District
K. B. Nielson	Engineer	Providence District
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W. I. Kenerson	Asst. Technologist	Providence District
C. M. Hearn	Asst. Engineer	Providence District
R. S. Johnson	Junior Engineer	Providence District
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F. P. Fifer	Prin. Engineer	Binghamton District
J. L. Nathan	Sr. Engineer	Binghamton District
B. K. Hough, Jr.	Engineer	Binghamton District
R. M. Haines	Assoc. Engineer	Binghamton District
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J. A. Rooney		New York District
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F. L. Meara	Assoc. Engineer	Baltimore District
H. G. Groves	Inspector	Baltimore District
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	Great Lakes Division	
H. C. Woods	Sr. Engineer	Buffalo District
	Upper Mississippi Valley Division	
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P. B. Fleming	Lt. Col., C. E.	St. Paul District
J. O. Ackerman	Assoc. Engineer	St. Paul District
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E. E. Abbott	Sr. Engineer	Rock Island District
P. S. Reinecke	Lt. Col., C. E.	St. Louis District
E. M. Kniestedt	Sr. Engineer	St. Louis District
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C. Senour	Prin. Engineer	Division Office
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S. J. Buchanan	Engineer	

Name	Title	Office
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W. H. Jervis	Asst. Engineer	Vicksburg District
J. C. Baehr	Asst. Engineer	2nd New Orleans District
	Missouri River Division	
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W. F. Cummins	Engineer	Fort Peck District
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R. R. Philippe	Engineer	Pittsburgh District
S. D. Sturgis	Captain, C. E.	Huntington District
J. P. Hartman	Assoc. Engineer	Louisville District
	North Pacific Division	
E. E. Barnes	Captain, C. E.	Division Office
B. Torpen	Senior Engineer	Division Office
H. E. Brown, Jr.	Asst. Engineer	Division Office
F. S. Besson, Jr.	1st Lieut., C. E.	Portland District
J. D. Lang	1st Lieut., C. E.	Seattle District
	South Pacific Division	
B. F. Jakobsen	Senior Engineer	Division Office
B. C. Hill	Captain, C. E.	Sacramento District
L. W. Miller	Engineer	Los Angeles District
F. A. Robeson	Asst. Engineer	Los Angeles District
	Southwestern Division	
R. Hansen	Asst. Engineer	Little Rock District
J. D. Freeman	Jr. Engineer	Little Rock District
H. Kramer	Captain, C. E.	Conchas District
A. C. Flach, Jr.	Jr. Engineer	Conchas District
D. W. Persons	Asst. Engineer	Conchas District